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### Kelsey, Pappworth, and Beecher: Moral Awakenings

Hear now this, O foolish people, and without understanding; which have eyes, and see not; which have ears, and hear not.

—Jeremiah 5:21<sup>1</sup>

There are none so blind as those who will not see.

The most deluded people are those who choose to ignore what they already know.

—John Heywood, 1546

You know, there's none so blind as they that won't see.

—Jonathan Swift, *Polite Conversation*, 1738

Perception without conception is blind.

—Immanuel Kant, 1787, *Critique of Pure Reason*

In the fields of observation chance favors only the prepared mind.

—Louis Pasteur, December 7, 1854

There is none so blind

As He who will not see

—Ray Stevens, “Everything Is Beautiful,” 1970<sup>2</sup>

#### On Moralists' Perceptions of Immorality

As noted in chapter 1, Bentham and Moll were moralists. What makes moralists so intriguing is that they perceive as morally questionable, or immoral, what everyone around them does not find morally suspect or immoral. This chapter focuses on moralists in post–World War II America and Britain who viewed as immoral conduct that other biomedical professionals treated as morally acceptable. After a few preliminary remarks about the phenomenon of moral blindness and moral sightedness, the discussion focuses on three moralists, starting with a mature married woman who had the temerity to

defy conventional acquiescence to pharmaceutical companies by demanding that physicians who investigate new drugs obtain the informed consent of their subjects. It then turns to two other moralists: one, an uppity Jew with an acerbic personality and a penchant for media attention; the other, a poor boy from a hick town in middle America who rose to success by feigning to be a “Boston Brahmin”—a characterization of upper-class Bostonians at a time when, to quote a well-known limerick, “the Lowells talk only to Cabots, And the Cabots talk only to God.”<sup>3</sup> What these moralists have in common is that they perceived, recognized as immoral, and protested the immorality of studies that the editors and reviewers for prestigious medical journals, and their readership, never viewed as immoral—although, a decade or so later, these same studies became paradigms of immorality.

“Woke” is a word that twenty-first-century black protesters use to describe the phenomenon of moral awakening to something as immoral or unjust that one hadn’t previously perceived as such. University of California, Santa Barbara linguistics professor Deadre Miles-Hercules traces this use of “woke” to a 1923 collection of writings by Jamaican philosopher, social activist, and black nationalist, Marcus Garvey (1887–1940).<sup>4</sup> Miles-Hercules also found that famous blues singer Huddie (Lead Belly) Ledbetter (1888–1949) popularized Garvey’s use of “woke” in a 1938 song about the “Scottsboro Boys,” nine black men falsely accused and convicted of raping two white women.<sup>5</sup> White Americans became aware of this use of “woke” after the Black Lives Matter movement used it in a 2014 to protest police brutality and murders of black people. By 2017, the *Oxford English Dictionary* recognized this adjectival use of “woke” as a way of indicating that a person had become sensitive to, had been “awakened,” so to speak, to racial or social discrimination or injustice.<sup>6</sup> This usage seems closer to what Jamaican philosopher Marcus Garvey had in mind when he coined the expression in the 1920s (Romano 2020).<sup>7</sup>

As Garvey observed in a short essay, *Negroes Robbed of Their History*, “The white world has always tried to rob and discredit [blacks] of our history” (Garvey 2023, 29).<sup>8</sup> As Garvey might have anticipated, white conservatives soon robbed blacks of the word “woke,” appropriating it as a pejorative to derogate “injustices” arising from liberals’ reformist zeal. They inverted the meaning of “woke” to stand for something akin to “liberal political correctness,” a pejorative that, like “libtard,” is intended as both an insult and a parody. According to the *Cambridge Dictionary*, “libtard” is “a combination of ‘liberal’ and ‘retard,’ used to indicate a person so completely brainwashed by liberal ideas” that presenting alternative ideas to them is akin to talking to a mentally retarded person.<sup>9</sup> Whatever one’s

political beliefs, as the series of quotations prefacing this chapter indicates, the “libtard” phenomenon that conservatives experience in trying to talk to liberals (i.e., the phenomenon of “people, . . . which have eyes, and see not, which have ears, and hear not”) has been recognized by preachers, philosophers, prophets, and poets, over the ages. Some academic ethicists now characterize this concept as “moral blindness.”<sup>10</sup> Yet since recognizing moral blindness presumes an opposite state, it also must be meaningful to talk of the process of becoming morally sighted, that is, the phenomenon of becoming “woke.”

Two dictionaries responsive to linguistic innovations, the Urban Dictionary and Wiktionary, now recognize both “woke” and “unwoke.”<sup>11</sup> These antonym pairs and their more erudite cousins, “moral sightedness” and “moral blindness,” are used in the rest of this book to describe the perspectives of those who “see” or who do not “see” as immoral something that is later recognized as immoral by people in the same community. Wokeness has consequences; insofar as a community is morally awakened to conduct that it formerly deemed moral, its members are bound to ask philosopher Kwame Anthony Appiah’s (1954–) question, “What were we thinking? How did we do that for all these years?”<sup>12</sup> An important correlative of accepting the phenomenon of moral blindness and moral sightedness is the epistemological status of judgments of moral progress and regress as inherently perspectival. Thus, Brandt and Ramm were “woke,” or morally sighted, with respect to *Rassenhygiene*, seeing its implementation as moral progress and decrying bourgeois medical morality focused on individuals as morally blind because it put the interests of individual patients ahead of those of the *Volkskörper*. Conversely, Alexander, Ivy, and Pridham condemned followers of *Rassenhygiene* as morally blind, and their condemnation led to one of the foundational documents of modern medical ethics, the document now called “The Nuremberg Code.” What one can or cannot recognize as moral progress or moral regress depends on one’s vantagepoint—which is why conservatives complain of “libtardness” in response to liberals’ “wokeness.”

### Frances Kathleen Oldham Kelsey: The Female Conscience of the FDA

Moral sightedness is often the prerogative of outsiders accepted as insiders. A paradigm case is the Canadian American, Frances Kathleen Oldham Kelsey (1914–2015). Born and raised in Cobble Hill, a small town on Vancouver Island, Frances Oldham earned her Master of Science degree in pharmacology from McGill University. In 1936, just after the peak of the

Great Depression (1928–1933), Frances applied to a graduate program at the University of Chicago. By dint of excellent grades and a masculine-appearing first name, “Frances,” she bypassed the barriers of sexist gender discrimination and was admitted as a graduate student and research assistant.<sup>13</sup> She earned her PhD in pharmacology from the university and later participated in the research that became the basis for the 1938 US Federal Food, Drug, and Cosmetic Act (FD&C), the legislation that empowered the US Food and Drug Administration (FDA) to regulate the safety of food, drugs, and cosmetics. Frances Oldham added “Kelsey” to her last name in 1943 by marrying another physician, Fremont Ellis Kelsey (1912–1966). After joining the University of Chicago faculty, Frances earned an MD degree in 1950 and then served a two-year stint as associate editor of the AMA’s journal, *JAMA*.

She and her husband started a family in South Dakota, where they worked as general practitioners and where she also had an appointment as a university professor. Frances was later hired by the FDA as “one of only seven full-time and four young part-time physicians reviewing drugs” for the agency.<sup>14</sup> One month into her new job, she received her first assignment: an application to market a drug developed in 1954 that had been approved for sale in West Germany as a safe sleeping pill for pregnant women. Since the drug was also approved in Britain, Canada, parts of continental Europe, and South America, its approval seemed to be an ideal easy-peasy walk-in-the-park for a novice regulator. Yet, “The European data [submitted to the FDA] left [Kelsey] ‘very unimpressed.’ She had lived through cycles before in which a drug was acclaimed for a year or two—until harmful side effects became known.”<sup>15</sup> So Kelsey requested more data, either from existing studies or from new studies, on the drug’s impact on the fetus. In response, the company (Merrell Pharmaceutical, 1950–1996) ignored her request for additional data and instead blitzed her with testimonials from satisfied physicians. When Kelsey persisted in demanding more data, the company responded with a pressure campaign: “it made repeated phone calls and personal visits to Kelsey and complained to her superiors that she was unreasonable and nit-picking, and that she was delaying the drug’s approval unnecessarily.”<sup>16</sup>

Despite the pressure and personal harassment, Kelsey refused to approve the drug. Her concerns deepened in December 1960 when the *British Medical Journal* published a case report of neurological side effects from the drug. So, Kelsey again pressed the pharmaceutical company for more clinical data. As she explained in a May 5, 1961, letter, the FDA’s standard for approving new drugs should be that “the burden of proof that

the drug is safe . . . lies with the applicant.” “In this connection,” she continued, “we are much concerned that apparently evidence [of] peripheral neuritis [tingling in feet or hands] in England was known to you but not forthrightly disclosed.”<sup>17</sup> Angered by the accusation that Merrell had suppressed information, an executive from the company telephoned Kelsey’s supervisor threatening a lawsuit on the grounds that “he considered [the letter] somewhat libelous.”<sup>18</sup> Her supervisor supported Kelsey.<sup>19</sup>

About the same time physicians worldwide began to report a spike in miscarriages, neonatal deaths, and birth defects ranging from malformed internal organs, to eye or ear defects, to phocomelia (abnormally short limbs or flipper-like arms, often attached close to a newborn’s trunk). In November 1961, German pediatrician Widukind Lenz (1919–1995) determined that the cause of this outbreak of fetal congenital deformations was thalidomide, the very drug the American pharmaceutical company was pressing Kelsey to approve.<sup>20</sup> In an odd twist of history, Widukind was the son of a Nazi party member Fritz Lenz (1887–1976), one of the foremost theorists of *Rassenhygiene* and coauthor of the standard textbook on the subject.<sup>21</sup> Following in his father’s footsteps, Widukind had been a leader in the Hitler Youth and had joined Hitler’s “Brown Shirt” paramilitary organization (SA). After the war, both father and son were “rehabilitated.” Nonetheless, Fritz continued to espouse eugenicist ideas about the need to prevent the propagation of *Lebensunwertes Lebens*. His son, Widukind, inherited this familial obsession but became a researcher focused on preventing fetal anomalies. This concern motivated his identification of thalidomide as the cause of phocomelia and other congenital deformities. Alerting the world, he prevented disability and deaths in thousands of unborn children of all races. Unfortunately, an estimated 8,000 infants, worldwide, had already been born with thalidomide-induced missing or malformed limbs; another 5,000 to 7,000 are estimated to have died in utero. The United States was a notable exception. Thanks to Widukind Lenz’s research and to Kelsey’s timely refusal to approve thalidomide, only seventeen American-born children are known to have suffered congenital defects from the 2.5 million “investigational” samples of thalidomide tablets that Merrell Pharmaceutical supplied to 1,267 American physicians.<sup>22</sup>

Fortuitously, Kelsey’s actions caught the eyes of the staff of Democratic Senator Estes Kefauver of Tennessee (1903–1963, senator 1949–1963). The senator had been investigating pharmaceutical companies’ pricing practices since 1959 but he could not get a bill through Congress. Seeking to spark support for the senator’s flagging legislative initiatives, his staff leaked information about Kelsey’s refusal to approve thalidomide to the

*Washington Post*. The July 15, 1962, edition of the *Post* headlined on its front page, “‘Heroin’ of FDA Keeps Bad Drug Off Market.”<sup>23</sup> The resulting near-scandal reenergized Kefauver’s mordant efforts to reform the pharmaceutical companies, culminating in the passage of 1962 Kefauver–Harris Amendment to the Federal Food, Drug, and Cosmetic Act. This amendment supported as official FDA policy Kelsey’s view that pharmaceutical companies had the burden of proving to the FDA that novel drugs (and vaccines) were safe and effective. Moreover, at Kelsey’s urging, for the first time in US history, the law also required that investigators seeking FDA approval for new drugs obtain the informed consent of their human subjects.

In some autobiographical remarks, Kelsey recollects her struggles with what bioethicists would later characterize as “scientistic medical paternalism,” that is, the parental-like authority over all aspects of medicine accorded to physicians due to their scientific knowledge and medical experience. As Kelsey explains to an interviewer,

The 1962 Kefauver-Harris Amendments and the 1963 investigational drug regulations introduced a number of new procedures which led to the strengthening of the control of drugs entering the market in the United States. . . . One very dramatic last-minute addition to the 1962 amendments was by Senator Jacob Javits of New York. He had raised the question, “Do people know they are getting investigational drugs?” It was very clear from our survey of the 1,000 doctors in the thalidomide case that many of the mothers and patients had not been told this, and the doctors themselves did not quite understand the status of the drug. So, a very important amendment to the law, not a regulation, was that patient consent must be obtained before a new drug, an unapproved drug, was given in a clinical trial.

. . . the statements in the 1962 law and the [1964] Food and Drug regulations . . . are exactly the same. They used the same words, because frankly this was a new concept for the Food and Drug Administration. We never imagined we could have gotten away with anything, however much we thought the doctor should do [it], because at that time the doctors felt they were the Lord Almighty. That the patient should take what the doctor gives them because doctor knows best. And if the doctor thinks it is important that this drug be studied in a fashion that the patient does not know he is getting an unproven drug—not to worry. Big Daddy will take care of you.<sup>24</sup>

More specifically, in responding to Senator Jacob Javits (1904–1986, senator 1957–1981), Kelsey suggested language from a 1962 draft of the Declaration of Helsinki that required that investigators obtain the informed consent of research subjects receiving experimental drugs. In the words of the 1962 statutes, repeated in the 1963/1964 FDA regulations,

Experts using such drugs for investigational purposes certify to such manufacturer or sponsor that they will inform any human beings to whom such drugs,

or any controls used in connection therewith, are being administered, or their representatives, that such drugs are being used for investigational purposes and will obtain the consent of such human beings or their representatives, *except where they deem it not feasible or, in their professional judgment, contrary to the best interests of such human beings.* (Italics added)<sup>25</sup>

Although the statute restates the consent requirement in the 1962 draft of the Declaration of Helsinki in legislative language, the italicized words created a loophole big enough to drive the proverbial truck through. At this moment, Kelsey was also urging officials at the National Institutes of Health (NIH) to promulgate a similar regulatory requirement since “there is no generally accepted professional code [of ethics] relating to the conduct of clinical research [this is] a mounting concern . . . over the possibility of untoward events . . . [because] highly consequential risks are being taken by individuals and institutions as well as NIH” (the National Institutes of Health, founded 1887).<sup>26</sup> Nonetheless, scientism triumphing over ethics, the NIH declined to require informed consent on the grounds that “whatever the NIH might do by designing a code or stipulating standards for acceptable clinical research would be likely to inhibit, delay, or distort the carrying out of clinical research rendering any such effort unacceptable,”<sup>27</sup> that is, scientific visions of medical progress overrode ethical concerns.

In 1962, President John F. Kennedy (1917–1963) awarded Kelsey the Medal of Freedom, and the popular press knighted her the “Feminine Conscience of the FDA” and “Guardian of the Drug Market.”<sup>28</sup> One issue that still vexed Kelsey, however, was the loophole in the law that its opponents had created to allow investigators to waive the requirements for obtaining and documenting informed consent. In 1963, she insisted that the grounds for waiver were very narrow. In a memo, she

made it clear that such exceptions to the obtaining of patient consent might include unconscious patients; a child in an emergency if the parents cannot be reached; mentally incompetent patients with no known representatives; or patients suffering from an incurable disease when the doctor feels knowledge of the nature of such disease would be detrimental to the welfare of the patient. There is no known basis for concluding that such exceptions would include circumstances in which the investigator feels that informed consent would interfere with the design of the experiment or would disturb the “doctor–patient” relationship.<sup>29</sup>

Thus, it was Kelsey’s official opinion that the only exceptions to the consent requirement were (1) the infeasibility of obtaining valid consent, as in emergencies, or (2) therapeutic experiments in which requesting informed consent would be detrimental to the welfare of a patient.



In 1964, Henry Beecher, who championed professional self-regulation, but abhorred the notion of bureaucratically imposed rigid rules, and seemed fearful that informing patients might lead to disruptive selection (i.e., by discouraging some types of potential subjects from participating), wrote to the FDA commissioner “contesting Frances Kelsey’s authority.”<sup>30</sup> As these criticisms mounted, Kelsey “lost formal control over investigational drugs and suffered what one reporter would later describe as a ‘humiliating *bare desk* treatment, she was generally ignored and given little to do of consequence.’”<sup>31</sup> Nonetheless, some of the specialist literature on the regulation of research on human subjects recognized Kelsey’s pivotal role in fighting to require that investigators for pharmaceutical companies document having the informed consent of the people on whom they tested experimental drugs.<sup>32</sup>

On August 30, 1966—that is, after the publication of Beecher’s 1966 whistleblowing *New England Journal of Medicine* article, “Ethics and Clinical Research”—the FDA published “A Statement on Policy Concerning Consent for the Use of Investigational New Drugs on Humans,” initiating Kelsey’s proposal that exceptions to the consent requirement were very narrow. Furthermore, in the wake of Beecher’s whistleblowing article, the NIH joined the FDA in officially recognizing a potential conflict of loyalties and interests between physician-investigator roles as researchers pursuing science (and their own career interests) and as physicians acting in the interests of their patients. Thus, in the mid-1960s, well before the conception or birth of bioethics, both the FDA and the NIH held that in the clinical “setting in which the patient is involved in an experimental effort, the judgment of the investigator is not sufficient as a basis for reaching a conclusion concerning the ethical and moral set of questions in that relationship.”<sup>33</sup>

In chapter 11, I will reflect on why Kelsey’s role as a bioethics reformer is barely acknowledged in standard histories of bioethics. I will contend that, among other things, acknowledging Kelsey’s role would have highlighted the role of the WMA’s 1962–1964 Declaration of Helsinki, thereby undercutting the presumption of American exceptionalism that runs through Jonsen’s and Rothman’s standard histories of bioethics. Moreover, it is also clear that governmental regulation of research on human subjects by the FDA and NIH was prior to and thus not a product of the creation of bioethics. Kelsey is the mother of these regulations, and her successful efforts to require documented informed consent in law and regulations in 1962–1964 came to fruition prior to the formation of the Society for Health and Human Values (SHHV, founded 1969) and first bioethics institutes (founded 1969 and 1971), prior to the Library of Congress’s



formal acknowledgment of bioethics as a separate field (1974), and prior to the formulation of bioethical principles for human subjects research in the 1978 *Belmont Report*. Perhaps Kelsey's contributions were ignored because she was female, and acknowledging her femininity, or her role as the martyred mother of informed consent, might clash with masculine conceptions of bioethics' founders as "fathers" of the field. Females saving babies can be accepted as a properly feminine role; a female as a heroine of research ethics reform would place her outside of a woman's submissive feminine role. Discussion continued in chapter 11.

In 2001, forty years after the *Washington Post's* thalidomide story, a surprising new voice, the AMA's *Virtual Mentor* (founded 1999, since 2015 renamed the *AMA Journal of Ethics*) crowned Kelsey a heroine not of bioethics but of professional medical ethics. In an article lauding Kelsey for her "commitment to the professional ideal of patient health and safety," she was styled "a role model in medicine."<sup>34</sup> Yet this laudatory narrative left untold the dark side of Kelsey's encounter with Lords Almighty physicians and the pharmaceutical industry. It failed to mention Beecher's campaign, which, with the help of the business press, sidelined Kelsey, demoting her to a "bare desk bureaucrat." Nonetheless, it is impressive that a female fellow of the AMA's Ethics Standards Group, Karen Geraghty, recognized Frances Oldham Kelsey's well-deserved status as heroine of biomedical ethics by crowning her a role model for professional medical ethics.

### Maurice Pappworth, Whistleblowing Moralist

Coincidentally, in the same year that Kelsey received her medal, 1962, a British physician, Maurice Pappworth, published an article revealing researchers' abusive use of patients as "human guinea pigs," in the British National Health Service and in the United States. Pappworth was a veteran who served in the Royal Army Medical Corps (RAMC) in Greece, Italy, and North Africa during World War II and who oversaw a military hospital in India during the immediate postwar period. Upon returning to Britain, however, Pappworth, was unable to get an appointment at a London teaching hospital. So, he became a Harley Street private practitioner who gave lectures and tutorials to postgraduate students. It was these students who made him "woke." As Pappworth tells the tale,

Several of [his] postgraduate students . . . told [him] about unethical experiments that they had personally observed in British hospitals in which they were either junior staff or attending courses. Some told me of their dilemma: Whether or not to take an active role, or even a passive one, in persuading a patient

to volunteer, knowing that noncooperation might jeopardize their careers. For many years as background to my tutorials I had spent hours in the Royal Society of Medicine library scanning journals in which experiments in humans were described that seemed to be unethical and sometimes illegal. A further concern was that promotion in teaching hospitals depended primarily not on clinical or teaching ability but on published work. . . . So, whenever I read an account of an unethical experiment, I wrote a letter to the journal protesting, often as not to have it rejected. Medical research had become sacrosanct, based on the dubious dogma that its continuation must be the prime concern of teaching hospitals.<sup>35</sup>

Several points are worth noting about Pappworth's account of becoming woke: first, the students, as novices, having yet to become acculturated into the ethos of hospital practice, could still see that their professors were asking them to be complicit in morally questionable activities. Like the child in the tale of the unclothed emperor, the unacculturated students could see naked immorality, whereas their acculturated professors, morally blinded by the culture of academic medicine, could not. Second, when the students reported what they had seen to Pappworth, he took their concerns seriously enough to investigate and confirm their observations. Pappworth also acted on their concerns by writing letters critical of these experiments to the editors of the journals in which they were printed.<sup>36</sup>

Pappworth's letters were typically rejected, and he came to believe that editors were rejecting his letters because "medical research had become sacrosanct, based on the dubious dogma that its continuation must be the prime concern of teaching hospitals."<sup>37</sup> Yet another factor in these rejections was Pappworth's ungentlemanly blunt language: his tendency to call experimenters "dastardly," or to describe British teaching hospitals as "dominated by ghoulish physiologists masquerading as clinicians," or to call for researchers to be replaced by "true physicians whose main interest is the welfare of their patients and not the publication of papers."<sup>38</sup> As one editor wrote to Pappworth, "We cannot accept [this letter] in its present form," explaining, "I know that there are times when good comes of speaking strongly and by giving maximum publicity to what appear to be public scandals; but you haven't persuaded us here that this is one of those occasions."<sup>39</sup> Note that the editor seems more affronted by Pappworth's strong language than by his reports of an ethical transgression.

As Pappworth's daughter and biographer, Lady Joanna Seldon, PhD (1954–2016), wrote, "Many British doctors during the 1950s and 1960s, though aware of the unethical research taking place in this country, nevertheless turned a blind eye to it."<sup>40</sup> Frustrated by journal editors' refusal to publish his letters, Pappworth writes that when

in 1962 I was approached by the editor of *Twentieth Century* (a now defunct literary magazine) to contribute to a special number entitled “Doctors in the Sixties.” [He published an] article, called “Human Guinea Pigs: A Warning” [that] appeared in the autumn of 1962, describing 14 experiments in lay language; no names were named, and no journal references were given. This was an early event in the debate in Britain about human experimentation. The debate excited much interest in both the lay and medical press and led eventually to the establishment of local medical research ethics committees.<sup>41</sup>

In this initial article, Pappworth cited fourteen cases, ten British, four American.<sup>42</sup> And, like Moll, his long-forgotten German precursor, Pappworth chose not to disclose the names of the researchers who performed these unethical experiments, nor their institutions nor the names of the journals that had published them. Nonetheless, again like Moll, Pappworth was soon vilified by his medical peers—in this case, the British medical establishment, which wrote him off as a noisome disloyal crank. Unlike Moll, however, Pappworth’s whistleblowing article goaded other physicians to action. The first of these was an eminent physician, Sir Hugh Anthony Clegg, editor of the *British Medical Journal*. In the October 1962 issue of the journal, Clegg, noting that Pappworth’s warnings and Kelsey’s actions had “recently received a lot of attention in the press in this country [Britain] and in other countries [the US], it is thought desirable that the medical profession be made aware of what progress [the WMA Committee on Medical Ethics] has made in this admittedly difficult subject.”<sup>43</sup> This said, Clegg published, without authorization, a revised working draft of the 1962 version of the document that would, after further revision, become the WMA’s 1964 “Declaration of Helsinki.”<sup>44</sup> By his unauthorized disclosure, Clegg made the scandal-to-code connection by relating the need for an ethics code to address the scandals revealed by Kelsey and Pappworth—invoking an aura of notoriety to pressure the WMA to get on with the unfinished business of finalizing the world’s first operational code of ethics for morally permissible experimentation on human subjects.<sup>45</sup>

### Henry K. Beecher, Whistleblowing Moralist

On the other side of the Atlantic, Pappworth’s publication, “Human Guinea Pigs—A Warning,” attracted the attention of Andrew Ivy, coparent of the Nuremberg Code. Ivy initiated correspondence in which he supported Pappworth’s efforts to draw attention to unethical research.<sup>46</sup> So too did another American physician, Harry Unangst (1904–1976), of

Peck, Kansas, a small town fifteen miles from Wichita. Like Pappworth, Harry was a veteran who had served in field hospitals in North Africa and Europe during World War II. And, again like Pappworth, he was not born with the proverbial silver spoon in his mouth. His father, Henry Eugene Unangst, had trouble keeping a job and had a weakness for gambling and alcohol.<sup>47</sup> Henry's grandson, Jonathan, reports that his fraternal grandfather was "a 'scoundrel'—[who spent] much of his time, and the family's money, drinking, gambling, and philandering. Dad, I think, never forgave his father for these things and especially for cheating on his mother."<sup>48</sup> By default, many familial responsibilities fell on the shoulders of young Harry, a book-worm who managed to amuse his friends by playing ragtime and popular tunes on the piano as he worked to pay his way through high school and college—his ne'er-do-well dad, Henry, having refused to support him.

Against the odds, Harry overcame these obstacles to become the first member of his family to graduate college, earning BA and MA degrees from the University of Kansas (1926, 1927). At college, one biographer observes, Harry

developed a foppish side—[Johnathan] recalls a "rather dapper photo of him, tennis racket in hand, from about that time" . . . evidence, perhaps, of a protective veneer, an overdone trust in appearances, an unsettling sense of vanity. Or perhaps it reflects a wish to distance himself from his father. . . . He was remarking himself, fashioning a new self-image, not for the first time and certainly not for the last. Just a couple of years later—as early, perhaps, as 1924, his junior year—he began experimenting with changes to his name, the formulation *Harry K. U. Beecher* arising from the erasure marks over "*Harry K. Unangst*."<sup>49</sup>

Harry's experiments with nominal self-reinvention led him to seek to change his name in the summer of 1928, when, on the verge of applying to Harvard Medical School, he legally adopted as his family name the maiden name of his maternal grandmother, Mary Julian Kerley née "Beecher" (1807–1885). In September 1928, a court granted his request and Harry applied to Harvard Medical School using the pared-down version of his new name, "Henry Knowles Beecher."<sup>50</sup> Harry had nominally rebirthed himself as an apparent descendant of Presbyterian minister Lyman Beecher (1775–1863), whose progeny included the famous abolitionist Henry Ward Beecher (1813–1887) and his even more famous sister, Harriet Beecher Stowe (1811–1896), author of *Uncle Tom's Cabin*. Having undone his plebeian birthright by rejecting the name of his philandering wastrel alcoholic father, Harry embraced his new persona and his fabricated "Beecher" heritage for the rest of his life—which forced him, as one biographer noted, to

cultivate a “penchant for deflection and evasiveness in discussions of his family history.”<sup>51</sup>

As Henry K. Beecher, Harry became “a fixture of Boston society, fond of ballroom dancing, lobster, and fine Scotch, the latter often consumed in generous quantities at the Somerset Club, an exclusive WASP redoubt on Beacon Hill”: that is, he became a Boston Brahmin.<sup>52</sup> Combined with his ambition, talent, charm, and hard work, Harry’s new persona transformed a life once foreordained for obscurity into one bound for the limelight. After graduating from Harvard Medical School, Harry, now known as “Henry Knowles Beecher”—but still “Harry” to his friends—climbed the professional ranks to become the Henry Isaiah Dorr Professor of Anesthesiology at Harvard Medical School. In 1934, he married an obstetrician’s daughter, Margaret Swain (1906–1973), an elegantly slim young woman who belonged to a fashionable charity society, the Boston Junior League. When his country called during World War II, Harry served honorably, receiving five battle stars.

Beecher was honorably discharged at the rank of lieutenant colonel, and he had been awarded a silver star medal for his heroism tending to American and allied troops during the bloody battle of Anzio, Italy, where about one-third of the US and Allied troops were killed or wounded in the battle (circa 40,000 casualties). He was called back into service by the US Army and the CIA, which tasked him with evaluating data from the Nazi doctors’ experiments and then with advising on and performing secret experiments on performance-enhancing drugs and with evaluating mescaline and LSD as potential truth serums. The context for these requests was Prime Minister Winston Churchill’s 1946 “Iron Curtin” speech, followed by a battle of wills in the 1948–1949 Berlin airlift, exacerbated by the Soviets’ explosion of a nuclear device (a version of an atomic bomb) in Semipalatinsk on August 29, 1949. The Cold War threatened to turn kinetic as the Soviet Union undermined and replaced governments throughout Eastern Europe and as a communist army took over mainland China. These events led to the formation of the NATO alliance and set the backdrop against which the CIA and US military exploited Beecher’s military experience and neuroscientific fascination with mind–body relationships (as expressed, for example, in his 1955 paper, “The Powerful Placebo.”<sup>53,54</sup>) to recruit him as a scientific investigator.

As required of all US government-sponsored researchers in the pre-bioethical era. Beecher would have secured his subjects’ signatures on the “waiver and release” forms. These forms were designed to protect

researchers and their government sponsors and were often vague about the actual purpose or details of an experiment. More to the point, frontline field hospital physicians, like Beecher, had been accustomed to making such tough decisions as whether to sacrifice some wounded men to use scarce resources to restore health to those who could better serve military objectives by returning to the frontlines more rapidly.<sup>55</sup> As a world-renowned scholar on the ethics of military medicine observed, “The hallmark principles that drive bioethical decisionmaking in ordinary clinical settings are largely absent in military medicine. Military personnel do not enjoy a right to life, personal autonomy, or a right of self-determination to any degree approaching that of ordinary patients. . . . Military necessity grants paramount authority to reason of state, proportionality limits but does not eliminate excessive harm, and the doctrine of double effect permits unintentional harm to non-combatants.”<sup>56</sup> Insofar as Cold War researchers, like Beecher and his colleague Louis Lasagna, MD (1923–2003), believed the excuse of war applied to those working on the frontlines of the Cold War, they could, and did, assume the prerogatives afforded to frontline physicians during World War II.

Yet, troubled by his own conduct, Beecher began to discuss and to write about the ethics of research on human subjects. In February 1958, he sent a memo to Harvard’s Committee on Research indicating that neither Harvard nor “other similar institutions [have] faced up to the problems surrounding experimentation in man,” although he also believed that “these matters are too complex . . . to permit the establishment of rigid rules.”<sup>57</sup> Like many researchers in America during the Cold War period (1947–1991), Beecher believed that nobility of purpose framed in terms of some ideal, in this case, patriotism, sufficed to guarantee the morality of a well-constructed scientific experiment: a view that founding bioethicist Robert Veatch (1939–2020) characterizes as “social utilitarianism.”<sup>58</sup> On this view, if a well-constructed experiment holds the promise of yielding socially or militarily useful results, ethically questionable means of obtaining these results would be excusable. Nonetheless, to his credit, although he had accepted contracts from the US Army and the CIA to explore psychoactive drugs as possible truth serums, Beecher began to have moral qualms about his research. As neuro-anesthesiologist George Mashour observed, “It may appear paradoxical that Beecher, who advocated the ethical treatment of human subjects, had also engaged in potentially unethical work on hallucinogens for the government. A more compelling hypothesis, however, is that Beecher advocated ethical treatment of human subjects largely because of such work.”<sup>59</sup>

As Beecher's questions about the ethical conduct of experiments on human subjects intensified, he sought answers from a variety of sources. Lacking training in moral philosophy, Beecher, like other autodidacts, mixed and matched sources as they appealed to him, often with little rhyme or reason. Thus, as late as 1970, he quotes the following line from the English philosopher George E. Moore's (1873–1958) ideal utilitarian treatise, *Principia Ethica* (1903), as cited by situational ethicist Joseph Fletcher (1905–1991): “‘Right’ does and can mean nothing but cause of a good result’ and is thus identified with ‘useful’: whence it follows that the end always will justify the means, and that no action that is not justified by its results can be right.”<sup>60</sup> This quotation is cited as a prefix to a section subtitled “Situation Ethics,” in which Beecher describes an experience in North Africa when penicillin was in short supply and

hospitals were overflowing with wounded men. Many had been wounded in battles; many also wounded in brothels. Which would get the penicillin? By all that is just it would go to the heroes . . . who were still in jeopardy, some of whom were dying. They did not receive it; nor should they have; it was given to those in brothels . . . there were desperate shortages of manpower at the front . . . those with [serious wounds] would not swiftly be restored to the battle lines even with penicillin, whereas those with venereal disease on being treated with penicillin would in a matter of day . . . return to the front. . . . I believe that the course chosen was the proper one.<sup>61</sup>

Thus, Beecher believed that accomplishing socially useful ends may excuse a morally suspect means of achieving them. Beecher also cites with approval Fletcher's view that “*not only means but ends too are relative*, only extrinsically justifiable. They are good only if they happen to contribute to some other good than themselves.”<sup>62</sup> David Rothman notes that these ethical ruminations were designed to justify Beecher's own Cold War experiments, in which he had related to potential colleagues that he was “asked by the [US] Army to study compounds that . . . give access to the subconscious. The Army has a further interest as well: . . . Can one individual obtain from another, with the aid of these drugs, willfully suppressed information? If we undertake the study this latter question will not be mentioned in the contract application. We request that it is not referred to outside of this room”<sup>63</sup> (i.e., this was secret military research). Rothman notes further that Beecher expressed a belief common among World War II and Cold War researchers that, in Beecher's own words, “In time of war, at least, the importance of the . . . [a military] purpose hardly appears debatable.”<sup>64</sup>

As Beecher's colleague Louis Lasagna testified to President William Clinton's Advisory Committee on the Human Radiation Experiments



(1994–1995) from 1952 to 1954, Beecher and he had carried out US Army–sponsored hallucinogen experiments on uninformed Harvard students.

The idea was that we were supposed to give hallucinogens to healthy volunteers and see if we could worm out of them secret information. And it went like this: a volunteer would be told, “Now we are going to ask you a lot of questions, but under no circumstances tell us your mother’s maiden name or your social security number.” . . . We’d give them a drug and ask them a number of questions and sure enough one of the questions was “What was your mother’s maiden name?” . . . The subjects were not informed about anything. . . . [We felt that] if we ask for consent, we lose our subjects . . . we were so ethically insensitive that it never dawned on us that you ought to level with people that they were in an experiment.<sup>65</sup>

By sheer happenstance, one of the Harvard students who served as an underinformed subject in another Beecher–Lasagna experiment was none other than Daniel Callahan, cofounder of the first bioethics thinktank, the Hastings Center. As Callahan tells the tale, when he was

a grad student at Harvard in 1960 or so, a notice appeared in the paper that a research project was beginning that called for the participation of former swimmers and runners. I had been a swimmer in college. We were not told the goal of the research or who was running it. . . .

We were not told what the research was about, nor did I know the name of the person directing the swims; and I don’t think there was any informed consent. [The published paper states that “The subjects were told that ‘pep pills’ and placebos were being used.”]

Then, around 1971 or so, I read Beecher’s CV since he was part of [the Hastings Center’s] project on brain death. I noticed in his list of publications a project he had run on amphetamines that involved swimmers and runners in the early 1960s—and that’s how I first learned what the research was and who had run it! Sydney [Dan Callahan’s wife] recalls it well because I came home after the trials with the faster second dose [of an amphetamine] high, happy, and agitated—and then in a few hours became very nasty.<sup>66</sup>

Despite this discovery, Callahan continued working with Beecher and, in recognition of Beecher’s role as a whistleblower and medical ethics reformer, established the Henry Knowles Beecher Award in medical ethics in 1976, making Beecher himself its first awardee.

As to Beecher, after conducting these experiments, he began writing articles in which he attempted to reconcile them with his conscience. The moral dicta that Beecher typically quoted seemed to justify serving one’s country by giving amphetamines or LSD to underinformed Harvard students in secret military-financed experiments. Quotations from Fletcher’s situational ethics appealed to Beecher because they emphasized that an

operative morality had to be flexible, because moral “matters are too complex . . . to permit the establishment of rigid rules.”<sup>67</sup> Yet, in a consummate example of trying to have one’s conceptual cake after having eaten it, Beecher follows these observations by invoking Immanuel Kant’s (1724–1804) dictum that “people must always be treated as ends, never as means alone.”<sup>68</sup> As anyone who has ever taken an introductory ethics course could have informed Beecher, this meant that one may not use uninformed and unconsented people as a mere means to find out about the military utility of psychoactive drugs. Yet Beecher reiterated his Kantian leanings by quoting a later statement of neo-Kantian ethics by British-Polish-Jewish mathematician Jacob Bronowski (1908–1974), “that the end for which we work exists and is judged only by the means we use to reach it.”<sup>69</sup> The fact that these statements about ends and means are inconsistent with each other does not seem apparent to Beecher. Perhaps, at some level, he was conscious of his inconsistencies because he ends this chapter by quoting a line Rainer Maria Rilke’s *Letter to the Young Poet*: “That we must seek to do the difficult is a certainty that may never leave us.”<sup>70</sup>

Quite inadvertently, Beecher seems to be embracing the views of Ralph Waldo Emerson (1803–1882), the Sage of Concord, who wrote, “A foolish consistency is the hobgoblin of little minds, adored by little statesmen and philosophers and divines. With consistency a great soul has simply nothing to do. He may as well concern himself with his shadow on the wall. Speak what you think now in hard words, and to-morrow speak what to-morrow thinks in hard words again, though it contradicts everything you said to-day.”<sup>71</sup> One virtue of a self-contradictory ethics is that it enables one to endorse actions inconsistent with some of its precepts. Beecher’s inconsistencies troubled Robert Veatch, a founding bioethicist well versed in moral philosophy. Veatch observed that although Beecher seems to have embraced some form of situationist utilitarianism (hence the quotations from Fletcher and Moore and the story about rationing penicillin), he inconsistently dismisses as “a pernicious myth [the] view that ends justify means” in research, claiming that “a study is ethical or not at its inception. It does not become ethical merely because it turned up some valuable data.”<sup>72</sup> Veatch concludes it “sounds like [Beecher] has some criterion of ethical rightness, other than social utilitarianism,” but what this was remains a puzzle.<sup>73</sup>

I think Veatch gives Beecher’s amateur philosophizing too much credit. In fact, Beecher’s publications and his private letters of the 1960s and 1970s reveal a man perpetually ambivalent. Sometimes he writes like the scientific investigator who surreptitiously gave psychoactive drugs to

unsuspecting people on the grounds that it could serve country, medicine, science, or society—and/or satisfy his curiosity about mind–brain interactions. At other times he is the observant bible-reading full-immersion Congregational Methodist who takes to heart St. Paul’s admonition not to do evil that good may come of it (Romans 3:8). He seems to have lived a life of situational ethics in which one adjusts one’s name and persona—and one’s ethics—to the needs of the moment, finding supportive quotations to justify whatever ideas or actions he has in mind, even if they are inconsistent with each other. Although Beecher is unlikely to have read the preface to English philosopher F. H. Bradley’s 1893 book *Appearance and Reality*, he, like Bradley, may believe that philosophy “is the finding of bad reasons for what we believe upon instinct.”<sup>74</sup> Yet, throughout his life, it was instinctively important to Beecher to do, and to be seen as having done, the right thing: to stand by his family, his faith (he never abandoned the Congregationalist Methodist church), his profession, the ideal of professional self-regulation—and to serve his country.

### Beecher and Pappworth’s Correspondence

Andrew Ivy was not the only physician on the other side of the Atlantic to notice the notoriety surrounding Pappworth’s 1962 article in the popular quarterly literary magazine, *Twentieth Century*. Henry Beecher also contacted Pappworth, and although the two were physically, religiously, and professionally worlds apart, for a time they became intercontinental pen pals. It was an odd correspondence. On one end was an orthodox Jew who had assumed the role of Old Testament prophet chastising the sinful for their sins by publicly condemning researchers for performing morally dubious experiments on unsuspecting patients without their consent; on the other was a Congregationalist Methodist struggling with guilt feelings about having committed unethical experiments in the interests of national security. Yet, like Pappworth, Beecher came from a strict religious tradition disdained by their fellow physicians (a form of Congregationalist Methodism that involved baptism by full emersion in bodies of running water like rivers and streams). Both had done exceptionally well in school: Pappworth earned degrees in both medicine and surgery from a newly founded red brick state school, the University of Liverpool; Beecher earned a bachelor’s degree and master’s from the University of Kansas (1926, 1927). But the two men had more in common than plebian origins and a disdained religious background. Both had undergone nomological reinvention. Pappworth’s family of Polish-Jewish immigrants had given him the

Hebrew name, “Moshe [Moses] Elkanan ben [son of] Yitzakh Yaakov ve [and] Miriam Devorah.” To pacify the dictates of Russifying governmental authorities in Poland, the family had added a Russian-sounding name “Papperovitch.” When the family moved to Britain it Anglicized this name for reasons of acculturation, and so “Moishe Papperovitch” was renamed “Maurice Pappworth.”

Pappworth, like Beecher, had served in the brutal North Africa campaign during World War II and was honorably discharged at the rank of lieutenant colonel. Unlike Beecher, however, when Pappworth returned to London after the war, he was not welcomed into an academic medical post. Instead, he was refused appointments at London teaching hospitals, apparently because of anti-Semitic prejudice. Pappworth’s path to career success had been blocked when he first moved to metropolitan London in 1939 because “deficiencies” in his background and education were made evident to him and he was barred from appointments to London teaching hospitals because, he was informed, such positions were reserved for gentlemen and “no Jew could ever be a gentleman.”<sup>75</sup> Forced into private practice, Pappworth supplemented his income with lectures that prepared medical students to take qualifying exams. As it happened, Pappworth’s outsider perch gave him the perspective to see what gentlemanly insiders could not: that some British physicians were using patients as unconsenting “human guinea pigs.”

Beecher first reached out to Pappworth in a January 7, 1965, letter on his official Harvard Medical School–Massachusetts General Hospital Henry Isaiah Dorr Professor of Research in Anesthesiology stationery.

Dear Dr. Pappworth,

I read with great interest, or perhaps I had better say I studied with great care, your interesting article, “Human Guinea Pigs: A Warning.”

... I heartily agree with your thesis and indeed have often spoken along the same lines myself.

As in most hospitals like the Massachusetts General Hospital we have had an explosion (I think no other term describes this situation) in research in man. As you have so well pointed out, this entails heavy responsibilities which are not always recognized. I am doing my best to see that they are recognized in this institution and for this reason would be especially grateful for your help.

... I would like a reprint [of your article] if you have one available.

Very sincerely

Henry K. Beecher M.D.<sup>76</sup>

Once Beecher and Pappworth discovered each other, Beecher began to look to Pappworth as a role model and soon adopted Pappworth’s method

of compiling lists of unethical experiments published in respectable medical journals. At this point, the mutual support team's correspondence focused on exchanging lists and recounting details of unethical experiments. It took a different turn after Beecher's January 25, 1965, letter to Pappworth. In that letter, Beecher explains that he is preparing a paper, "Ethics and the Explosion of Human Experimentation," in which he would report that at Massachusetts General Hospital, "there has been a 17-fold increase in funding [for research] in 20 years." Beecher would then suggest that this has led to a generational change leading to an exponential increase in unethical experiments: that is, an explosion of funding had attracted a younger generation of investigators who were more interested in publication than in patients' welfare. He thus concurred with Pappworth's publicly stated observations on generational change and a quest for publication as the source of the increase in unethical experiments.

Switching topics, Beecher exclaimed that in the previous year, he discovered "the most shocking example [of unethical experiments] has been described in great detail in *SCIENCE* magazine February 7, 1964, page 551, under the title 'Human Experimentation: Cancer Studies at Sloan-Kettering Stir Debate on Medical Ethics.'"<sup>77</sup> In a pivotal remark, Beecher observes that

certain elements in this country have long held the view that these matters must be swept under the rug. I do not at all agree with this point of view and I think with the "explosion" just referred to, that these matters must be faced. I have been invited to participate in a symposium for senior medical writers from the best newspapers and journals in this country and I expect to speak on the title mentioned above. . . .

While the individual who does it, well may be severely criticized, as I fully expect to be for presenting in the Middle West the paper I mentioned, I think that someone has got to call attention to the problems as they exist. It seems to me that a man must be prepared to stand behind the work he publishes; he must expect it will be referred to. Indeed, that is the purpose of publication.<sup>78</sup>

Beecher's letter ends with a discussion of journal editors' responsibility for monitoring the ethics of the research published in their pages. He notes that a recent survey of journal editors about their responsibility to monitor the moral status of the research reported in their journals found that while "fifty-eight per cent said 'yes, of course' [they had such a responsibility], an astonishing 28 per cent said 'no.' In the latter group one individual made the extraordinary comment '. . . the implications of effectively monitoring the standards of conducting experiments either with humans or animals would be highly antagonistic to the perpetration and expansion of the

research process.” In closing, Beecher reiterates the idea that “it does seem that some of us need to call attention to the not uncommon excesses.”<sup>79</sup>

What is evident from this letter is that, apparently scandalized by an experiment described in *Science*, Beecher was going to use his speaking slot at a midwestern conference attended by medical journalists to publicly call out unethical experiments on humans. The *Science* article that inspired Beecher to publicly denounce unethical research describes experiments conducted in 1963 by Dr. Chester M. Southam (1919–2002) of the Sloan Kettering Cancer Center and Cornell University Medical College. Seeking to challenge the then current view of cancer as a disease caused by external agents, like viruses, Southam conducted a series of experiments designed to test the hypothesis that cancers spread in people’s bodies because their immune system is unable to adequately reject them. As evidence for his idea that healthy people can reject cancer, Southam recruited fourteen healthy subjects from an Ohio Penitentiary who were “fully informed about the experiment and its possible risks and nonetheless eager to take part in the experiment in which they would receive injections of cancer cells.”<sup>80</sup>

At this point, according to the *Science* article, “the doctors faced a choice that has confronted researchers since the beginning of experimental medicine: Should they use themselves as subjects?” Some researchers, including Andrew Ivy, held that self-experimentation was a prerequisite for legitimizing potentially harmful experiments on other people, provided that potential subjects gave their informed voluntary consent. Words to this effect were included in his second draft of the Nuremberg Code and ultimately became Article 5 of the code, which states that “no experiment shall be conducted where there is an *a priori* reason to believe that death or disabling injury will occur; except, perhaps, in those experiments where the experimental physicians also serve as subjects.”

Post-Nuremberg, precedents for serving as a subject in one’s own experiments had been set in the mid-1950s when each of three polio vaccine researchers—Hilary Kropowski (1916–2013), Albert Sabine (née Abram Saperstein, 1906–1993), and Jonas Salk (1914–1995)—declared that they had taken their own polio vaccines as proof of their confidence in its safety.<sup>81</sup> Mindful of this precedent,

Sloan Kettering . . . issued a press release stating that the researchers did inject themselves with cancer cells and established the safety of the procedure before trying out larger-scale experiments at Ohio State Penitentiary. Southam, however, had been unwilling to inject himself or his colleagues when there was a group of normal volunteers . . . fully informed about the experiment . . . eager

to take part in it. “I would not have hesitated,” Southam said, “if it had served some useful purpose. But . . . to me it seemed like false heroism. . . I do not regard myself as indispensable . . . and I did not regard the experiment as dangerous. But, let’s face it, there are relatively few skilled cancer researchers, and it seemed stupid to take even the little risk.”

“Somewhere along the line,” the *Science* article continues,

the practice of fully explaining the experiment to the patients and obtaining their informed consent was replaced by the practice of obtaining oral assent only to a vague description of the procedures, in which the word “cancer” was entirely omitted and patients were merely told that they would be receiving “some cells.” . . . “We stopped telling them they were getting living cancer cells when it was well established that there was no risk,” Southam said last week. “. . . All I can say is that within any reasonable definition of the words ‘no risk’ there was no risk.” . . . To inform them more explicitly about the experiment, . . . To what purpose? I told them that they would be getting some cells, and I described what would happen, but—since I believed that there was no risk to them under the circumstances—to tell them the nature of the thing injected seemed irrelevant.<sup>82</sup>

By the summer of 1963, Southam had arranged with the research director of the Jewish Chronic Disease Hospital (founded 1925, Brooklyn, New York City) to conduct the experiment on twenty-two diseased noncancer patients. The diseased patients rejected the cancer implants just as promptly as healthy subjects did, providing further evidence supporting Southam’s hypothesis that the immune system was implicated in the human body’s susceptibility to cancer.

In August 1963, three physicians resigned from Jewish Chronic Disease Hospital in protest demanding an investigation by a medical committee. A committee was formed but, finding no ethical issues, commended the research instead.<sup>83</sup> The protesting physicians then contacted William A. Hyman (1893–1966), a New York lawyer who took the hospital to court, informing the New York newspapers of his lawsuit.<sup>84</sup> The *Science* article reports further that “it is now established that the Brooklyn hospital did not tell the patients that they were receiving cancer cell injections, and that they were not asked for written consent. [The] Hospital director . . . asserts that the patients were told they were receiving ‘some cells,’ and that they gave oral consent.”<sup>85</sup>

What was it about the case described in this article that led Beecher to describe it as “the most shocking example!” of unethical experiments on humans? What led to his exclamation point “(!)” or his statement that “some of us need to call attention to the not uncommon excesses?” No doubt Beecher, a faithful Methodist who read his bible regularly, was familiar with 1 John 19, “If we confess our sins, he is faithful and just to forgive



us our sins, and to cleanse us from all unrighteousness.” Given Beecher’s religious beliefs, some commentators suspect that, recalling that he himself had sinned, Beecher was seeking absolution. One typescript of his conference speech suggests that he contemplated playing the role of repentant sinner. In this typescript, Beecher wrote that he “was obliged to say that 17 years ago a group in [his] laboratory prolonged anesthesia 23 to 74 minutes beyond the necessary in order to study kidney function under ether as opposed to cyclopropane. The anesthetics were uneventful and no unusual strains were placed on the fifteen patients [crossed out: Some of those involved were minors.] . . . no adequate explanation was given to the subjects, nor was their consent adequately obtained. . . . All of this was quite wrong and I would not today, 17 years later, participate in such a study.”<sup>86</sup> Yet, as historian Susan Lederer observes, “in the version delivered at the conference Beecher . . . omitted specific reference to the anesthesia comparison, merely stating: ‘Lest I seem to stand aside from these matters I am obliged to say that in years gone by work in my laboratory could have been criticized:’ words that fall short of the full-throated confession of a contritely repentant sinner.”<sup>87</sup>

One can speculate that Beecher was reluctant to play the repentant sinner role because, when he shared a copy of his ethics speech with Louis Lasagna, his former research fellow, Lasagna responded, “Gee, Harry, when I was with you from 1952 to 1954, we never got consent on anybody.” Referencing the then standard waiver and release contracts required by government-supported research, Lasagna added, “We got releases from some of our healthy volunteers because we had an Army contract and the Army wanted to say if anything bad happened to them, they wouldn’t sue the Army, but that was it.” So, “[Lasagna] said, ‘How about . . . Let him who is without sin cast the first stone?’”<sup>88</sup> Beecher certainly had no wish to have his secret Army and CIA experiments with psychopharmaceuticals thrown back at him, so, perhaps he concluded that playing the penitent’s role would be counterproductive.

Then again, Beecher did not believe that every experiment on human subjects required the informed consent of the subject or a surrogate. Instead, Beecher believed as most researchers did prior to the bioethical paradigm shift, that the moral issue was informing potential subjects of risk of possible harms. In his view, the extent of information conveyed to subjects should be titrated to correlate with the potential risk of harm to which subjects might be exposed.<sup>89</sup> If the risk was slight, fully informed consent was unnecessary. Hence, in an era in which most psychedelic drugs were considered relatively harmless but potentially useful, perhaps Beecher

believed that informed consent was optional—and counterproductive if it discouraged people from volunteering as subjects. A similar line of argument was, in fact, Southam’s rationale for not asking for fully informed consent: that is, since, in his view, the cancer implants posed no risk whatsoever, there was no need for fully informed consent. As Southam put this point, “All I can say is that within any reasonable definition of the words ‘no risk’ there was no risk”—thus a full description mentioning the word “cancer” was unnecessary.

The catch, as became evident from the account in *Science*, was that Southam did not really believe that there was no risk. He explicitly forbade members of his research team to be injected with cancer cells, and he refused to have himself injected with them. As he put it, “Let’s face it, there are relatively few skilled cancer researchers, and it seemed stupid to take even the little risk.” “Little risk,” however, is not the same as “no risk.” More to the point, from Beecher’s Methodist perspective, Southam was violating the Golden Rule: he was not “doing unto others as he would wish others to do unto him” (Leviticus 19:18, Luke 6:31, Matthew 7:12). Worse yet, the real reason Southam avoided using the word “cancer” was, as Southam himself put this point, patients’ “bizarre, defensive reaction” to the word “cancer.” Given prospective subjects’ defensive reaction, using the scary words “cancer cells” would virtually guarantee their refusal. So, Southam committed what, in Beecher’s view, was a sinful action: telling an ignoble lie by omitting words indicative of risk of harm because he knew that potential subjects would not willingly subject themselves to this risk and then covering up this sin by seeking oral consent based on misleading descriptions of the risks involved (i.e., by deceiving patients). As Beecher, an avid and daily reader of the Bible would recall from Proverbs 6:16–17, among the things that “the LORD hates as an abomination unto him are, a lying tongue.”

Although something akin to these thoughts may have crossed Beecher’s mind, what appears to have motivated him most was the line in the *Science* article reporting that an “ad hoc medical grievance committee, convened by the hospital to investigate their charges, found no irregularities and instead commended the research.”<sup>90</sup> This clearly troubled Beecher, and he says as much to Pappworth.

Certain elements in this country have long held the view that these matters must be swept under the rug. I do not at all agree with this point of view . . . I have been invited to participate in a symposium for senior medical writers from the best newspapers and journals in this country and I expect to speak on the title mentioned above. . . . While the individual who does it, well may be severely criticized, as I fully expect to be for presenting in the Middle West

the paper I mentioned, I think that someone has got to call attention to the problems as they exist.<sup>91</sup>

Any one of these factors might have rendered the experiment scandalously immoral from Beecher's perspective. In combination, they overdetermined his description of Southam's cancer experiments as "the most shocking example" of an unethical experiment! Worse yet, in the Southam cancer case, a medical investigative committee had found no irregularities and instead commended the research. In the words of Jeremiah 5:21, they "have eyes and see not." To make sure that the public and their profession had eyes and would *see*, Beecher decided to follow Pappworth's example by speaking out in a forum that ensured media attention.

Pappworth had a similar experience. He too had been provoked into publishing "Human Guinea Pigs—A Warning" by journal editors' persistent refusal to publish his letters. It is important to underline the fact that *none* of the cases deemed "unethical" by Beecher and Pappworth were hidden from the medical community. They were published in such leading medical journals as the *Journal of the American Medical Association (JAMA)*, *The Lancet*, *New England Journal of Medicine*, and, even more publicly, the *New York Times*.<sup>92</sup> Beecher was, to reiterate his words in the January 25 letter, angry that "certain elements in this country have long held the view that these matters must be swept under the rug. I do not at all agree with this point of view . . . these matters must be faced. . . . I think that someone has got to call attention to the problems as they exist."<sup>93</sup> In actuality, of course, his task in "calling attention" was to make the medical community morally sighted to abuses in experiments that they were aware of, but to which they were morally blind, "unwoke," so to speak.

In a file at the Countway Library at Harvard Medical School, historian Susan Lederer discovered on "a yellowed sheet of paper in Beecher's handwriting . . . he had written 'Why me?'"<sup>94</sup> Beecher "offered three reasons. He was at the end of his career rather than the beginning. He could incur the risk of rocking the research establishment with his critique of medical research gone awry, because, at age 62, he was near retirement . . . and he was he had worked as both a clinician and an investigator on the wards of Massachusetts General Hospital [and was] qualified to offer his critique because he had worked as both a clinician and an investigator on the wards of the Massachusetts General Hospital. He had a well-established record of research."<sup>95</sup> So at the March 22, 1965, symposium on the "Problems and Complexities of Clinical Research," at Brook Lodge in Augusta, Michigan, Beecher intended to "call attention to the problems as they exist."

The conference setting, Brook Lodge, was a former summer home of the Upjohn family (owner of Upjohn pharmaceutical, now part of Pfizer) that had been repurposed for use as a corporate retreat. It had forty-eight guest rooms, a dining hall, and a conference center that offered guests a view of an 82-acre pastoral paradise. Beecher intended to disrupt this bucolic tranquility by calling out sinners. It was not his first attempt. He had been trying to alert fellow medical professionals to unethical experiments on humans since the late 1950s. Yet none paid attention. He is said to have exclaimed at one point that his efforts had “all the impact of a soap bubble.” So, emulating Pappworth, Beecher intended to publicize a Pappworth-style list of unethical experiments, focusing on the unethical cancer experiments described in the *Science* article.

Beecher had no training in moral philosophy, and not surprisingly, his ethical critique was an inconsistent jumble of ideas; nonetheless, he repeatedly stressed several points. First, he believed (concurring with Pappworth) that unethical experiments had become more commonplace because an abundance of government and pharmaceutical funding for research had drawn a new generation into the research enterprise that was more focused on finding publishable results than in protecting the welfare of their patients or subjects. Second, this new generation of investigators was “thoughtlessness and carelessness” but “not willful[ly] disregard[ing] . . . patients’ rights” in the manner condemned at the Nuremberg Trials.<sup>96</sup> Third, because patients had limited ability to understand or appreciate the possible risks and benefits of experiments, the best moral safeguard on experiments involving human subjects was prior ethics review by professionals like himself, “skillful, informed, intelligent, honest, responsible, compassionate, physician[s]”<sup>97</sup> who can understand and balance the beneficial ends that the experiment was designed to serve, against the dangers inherent in the means the experimenters had contrived to reach these ends—not rigid rules!

The wording Beecher, Kelsey, and Pappworth used to condemn unethical experiments indicates that their conception of the function of informed consent was what might be called “proto-bioethical.” In a context in which “lack of informed consent was a routine, though not universal feature of clinical research with patient-subjects”<sup>98</sup> these reformers sought to reshape subjects’ “waiver and release” consent forms designed to protect researchers, sponsors, and funders from lawsuits into something different: a shield against researchers’ attempts to exploit their subjects. The impetus for formalizing researchers’ moral responsibilities toward patients traces back to the Nuremberg Code, which made the subject’s informed voluntary consent its first principle. But this ideal received greater attention after 1952, when

it became the subject of Pope Pius XII's September 14 address to a medical audience in which he construed a patient's right to informed consent as a moral issue, not a legal matter. As the pope put this point, "As a private person, the doctor can take no measure or try no course of action without the consent of the patient."<sup>99</sup> About five months later, on February 26, 1953, Charles E. Wilson (1890–1961), secretary of defense (1953–1957) for President Eisenhower (US president 1953–1961), issued a memorandum, "Use of Human Volunteers in Experimental Research," requiring all US military-sponsored or military-funded research to conform to a version of some provisions originally stipulated in the Nuremberg statement of "Permissible Experiments on Humans" (later known as "The Nuremberg Code").

Beecher, however, objected on the grounds that "such a vast code would be restricting and crippling to experimentation in man."<sup>100</sup> He pressed his view that the "best protection" for human subjects was ultimately the medical researcher's "character, wisdom, experience, honesty, imaginative-ness and sense of responsibility."<sup>101</sup> Ultimately, Beecher and his Harvard colleagues forced the US Army to downgrade its demands for compliance with the Nuremberg principles to the status of mere "guidelines." Indicative of Harvard's prestige and of Beecher's deployment of that prestige, a US military that had demanded and received the unconditional surrender of the German and Japanese armed forces retreated in the face of a handful of Harvard faculty led by the Henry K. Beecher, the Henry Isaiah Dorr Professor of Anesthesia Research. Beecher's victory, however, would be short-lived since he himself would soon become woke to moral issues where he had once seen none.

### **Becoming Woke: Beecher and Pappworth as Moral Reformers**

In his study of scientific revolutions, Thomas Kuhn observes that revolutionaries are typically inspired by anomalies in conventional treatments of a subject. Had Kuhn written about moral revolutions in medical ethics, he no doubt would have noted that the two most effective whistleblowing moralists, Henry Beecher and Peter Buxtun (1937–), were also inspired by anomalies. Beecher says as much in the opening lines of "Ethics and Clinical Research," when he observes that "it must be apparent that [research subjects] would not have been available [for these experiments] if they had been truly aware of the uses that would be made of them. Evidence is at hand that many of the patients in the examples to follow never had the risk satisfactorily explained to them, and it seems obvious that further hundreds have not known that they were the subjects of an experiment although

grave consequences have been suffered as a direct result of experiments described here.”<sup>102</sup> This anomaly is evident in Beecher’s index case, Southam’s cancer implantation experiments. As reported in *Science*, Southam admitted that he never told his subjects that they were being injected with cancer cells because, “by any reasonable definition of the words ‘no risk,’ there was no risk.” And yet, according to the same article, Southam himself refused to be injected with cancer cells because, in his own words, “there are relatively few skilled cancer researchers and it seemed stupid to take even the little risk.”<sup>103</sup> As noted previously, “a little risk” that leads a fully informed person, like Southam, to reject an intervention is not “no risk.” So, Beecher concluded, as Southam himself no doubt did, that Southam’s subjects would not have participated in experiments had they been told that they were being injected with cancer cells. Of course, if Lasagna’s account of why Beecher and he failed to fully inform the subjects of their own psychoactive drug experiments is accurate, Beecher would have been familiar with this motive, since he had once disinformed his own subjects for the same reason.

Buxtun, like Beecher, was induced to act by an anomaly. He had been employed by the US Public Health Service (USPHS) to prevent the spread of sexually transmitted infections (STIs). His daily routine involved tracking down people who may have contracted syphilis or other STIs. So, imagine his shock when he learned that the USPHS itself was knowingly allowing hundreds of men with documented cases of syphilis, an STI, to spread the disease untreated and was actively preventing these men from receiving treatment. The USPHS’s Syphilis Study seemed self-evidently at cross-purposes with the USPHS’s basic obligation of preventing the spread of STIs.

Yet neither Beecher nor Buxtun was a moral revolutionary. Buxtun quit health care and did not foment transformational change after his interview with Jean Heller of the Associated Press and his testimony at the Kennedy hearings. Beecher sought to initiate reforms (informed consent, IRBs), but he believed in professional self-regulation and engaged in whistleblowing to spur fellow professionals to vigilance and to prevent jurispatic bureaucratic interventions that would undermine professional self-regulation. More personally, Beecher had no intention of undermining the scientific paternalist paradigm that gave him the academic status and the social prerogatives he enjoyed throughout his adult life.

Nonetheless, sometime in the 1960s, Beecher and Pappworth each became woke to the fact that the editors and readers of leading medical

journals were oblivious to unethical research routinely published on their pages. So, each undertook the challenge of awakening their colleagues to the unpalatably routine immorality of everyday research practices. Some people might characterize Beecher and Pappworth not as moral reformers but as whistleblowers. But, as used in this book, this characterization doesn't fit because the term "whistleblowers" designates people who alert the public to recognized wrongdoing: that is, to actions that the public understands to be morally wrong, such as frauds, or kickbacks. In the 1960s, however, neither the medical world, nor the public, nor the media recognized Southam's experiments—often described in the *New York Times*—as morally problematic. Thus, Beecher and Pappworth are best understood as moralists, like Bentham, since they too recognized and decried as immoral conduct that their colleagues did not recognize as immoral. Consequently, their initial attempts to awaken their fellow professionals had, in words attributed to Beecher, "little more impact than a soap bubble." Facing indifference or rejection, each of them decided to awaken their colleagues by turning to popular media.

Pappworth led the way with his article in *Twentieth Century*. Beecher was determined to do the same at the Brook Lodge Conference for science journalists. He circulated advance copies of his presentation to those planning to attend the conference and peppered his case descriptions with colorful remarks about experimenters who "evidently believe that they had the right to choose martyrs for science." His remarks were soon quoted in newspapers like the *Los Angeles Times* and in popular magazines like *Good Housekeeping*.<sup>104</sup> Blowback swiftly followed. Beecher had "the most humiliating experience": two Harvard Medical School faculty members, David Rutstein (1909–1986), a prominent physician and fellow veteran military physician (soon to head the Veterans Administration's research and education division) and host of the groundbreaking television series *Facts of Medicine*, joined with Thomas Chalmers (1917–1995) to call "a press conference to refute what [Beecher] had said without finding out whether or not [Beecher] could be present."<sup>105</sup> These eminent physicians publicly dismissed Beecher's cases as unrepresentative and noted that just because the published articles did not state that patients had given informed consent, it did not follow that the researchers had failed to obtain informed voluntary consent. Their press conference made national news. But not everyone turned against Beecher; someone wrote a letter to Beecher saying, "I want you to know how much I enjoyed the article [about you] and I know there are many more people who feel the same way. We need more, a



lot more Doctors like you who will tell the people the honest truth. But try and find them today. It is really sad. Thank you, Dr. Beecher, and continue to speak the truth. God Bless you always.”<sup>106</sup>

Following their efforts to use mass media to awaken the public to the scandal of the unethical abuse of patients as human guinea pigs, Beecher and Pappworth each sought to impart their message in some less ephemeral format: a journal article or a book. Since Beecher’s primary audience was his fellow physicians, he sought publication in a medical journal. Pappworth, having been denied professional platforms, sought to write a book for a lay audience. To achieve these objectives the two moralists would not only have to cope with reviewers’ unwoke hostility, they would also have to deal with the challenge of transforming their repetitive laundry lists of the unethical experiments into an engaging narrative.

Moreover, although each had a lifetime’s experience reading, teaching, and writing about medicine, neither had the literary skills, command of moral philosophy, nor the ethics lexicon needed for the task they were undertaking. Nonetheless, both moralists pursued publication. Beecher’s first thought was to send his manuscript to *Science*, the journal that published the article that catalyzed his decision to go public. Finding no encouragement in that quarter, Beecher next turned to *JAMA*, the journal that published his earlier reflections on the ethics of experimenting on humans—and the journal with the largest circulation. So, he sent a copy of his talk to *JAMA*’s editor, John Talbott (1902–1990), with a letter explaining that the manuscript summarized “about ten years of as careful thought as [he, Beecher] was capable of doing. [Moreover] many individuals . . . though appalled by the information, agree that it should be published, the sooner the better. . . . It is rather long. I do not believe it can be shortened significantly and carry the same message, which so urgently needs to be disseminated.” Talbott was receptive and, following standard procedure, informed Beecher that he had submitted his manuscript to several external reviewers. Alert to the possibility of blowback from unwoke reviewers, Beecher sent a note to Talbott indicating that “reverberations” from his Brook Lodge presentation “are still continuing. . . . Unquestionably the shoe pinched a lot of feet.”<sup>107</sup>

Beecher was right to feel apprehensive. None of the external reviewers favored publication. One wrote that the manuscript was so “poorly organized [that] frankly, I was surprised that a thoughtful physician of Doctor Beecher’s stature expected you to review this manuscript.”<sup>108</sup> The reviewers also rejected Beecher’s review of fifty cases of unethical experiments on the grounds that it exceeded the journal’s page limits and violated

the journal's editorial standards by not identifying the publications cited. Upon receiving the negative reviews, Talbott, no doubt mindful of the controversies surrounding Beecher's Brook Lodge presentation and Beecher's avowed refusal to pare down the number of cases, rejected the article outright (i.e., without encouraging revision and resubmission).

Beecher next approached Joseph Garland (1893–1973), editor of the oldest and, in the eyes of many, the most prestigious American medical journal, the *New England Journal of Medicine* (founded 1812). Garland sent the article to seven reviewers. Six recommended against publication of the article on the grounds that there were too many cases, that Beecher had not allowed investigators to tell their side of the story, that many medical readers would recognize the “anonymous” cases, and that Beecher's critique had already received extensive media coverage. Only one reviewer supported publication of the article as submitted, and then only on the condition that the *Journal* obtain a legal opinion “regarding any possible problems.”<sup>109</sup> Yet all but one reviewer believed the article merited publication in some revised form.

Some editors are transactional, submitting manuscripts for review and accepting the results. Great editors, like Maxwell Perkins (1884–1947) of Scribner Publishers, are transformational: discovering, mentoring, and standing by their authors even in the face of reviewers' objections. (Among Perkins's authors were F. Scott Fitzgerald, Ernest Hemingway, and Thomas Wolfe.) Garland was, by all accounts, a transformational editor; so, when his editorial board voted to reject Beecher's manuscript, Garland overruled it.<sup>110</sup> Still, a laundry list of fifty examples of unethical experiments would be a dull read with all the deficiencies cited by the reviewers. So, Garland wrote to Beecher requesting that he cut the number of cases in half. He also requested a list of references for all the experiments described so that they could be fact-checked against Beecher's accounts.<sup>111</sup> Ultimately, the paper was revised, and on March 3, 1966, Garland wrote to Beecher summarizing his editing of the revised paper:

The result of my editorial labor in which I have attempted to reduce your important data to a relatively unemotional statement of factual material. I have tried to omit anything accusatory or especially critical, since what we want is not an indictment but a sober and undramatic presentation of what has been done and is being done in violation of basic ethics.

To my mind this makes the message all the more impressive; *res ipsa loquitur*; [“*res ipsa loquitur*,” the thing speaks for itself].<sup>112</sup>

Some commentators believe that in the editing process, Garland blurred the line between editor and coauthor, but excision is editing, not

coauthoring.<sup>113</sup> More to the point, the content of the trimmed-down article is a faithful compendium of Beecher's thoughts during the decade in which he gradually became morally sighted.<sup>114</sup> The article opens with a statement from Beecher's January 7, 1965, letter to Pappworth, "Human experimentation since World War II has created some difficult problems with the increasing employment of patients as experimental subjects." It then continues,

In many of these experiments it must be apparent that patients would not have been available if they had been truly aware of the uses that would be made of them. Evidence is at hand that many of the patients in the examples to follow never had the risk satisfactorily explained to them, and it seems obvious that further hundreds have not known that they were the subjects of an experiment although grave consequences have been suffered as a direct result of experiments described here.<sup>115</sup>

Note the absence of the word "consent." For Beecher, the moral issue was not whether experimenters managed to get patients to sign a waiver and release document or signal oral consent or assent; he did not charge any researcher with the issue that his Harvard colleagues Chalmers and Rutstein harangued him with in their press conference. For Beecher, the moral issue turned on the question of whether patients had the risk satisfactorily explained so that they understood and appreciated the risks of harm to which they would be exposed if they agreed to become experimental subjects. Did they understand that they would be given "pep pills and a placebo," for example?

Beecher next paraphrases a line from his January 25, 1965, letter to Pappworth, "There is a belief prevalent in some sophisticated circles that attention to these matters would 'block progress.'" Beecher forthrightly rejects this view, citing a single moral authority, Pope Pius XII (the 1952 Encyclical), "who stated that . . . science is not the highest value to which all other orders of values . . . should be subordinated."<sup>116</sup> It is not clear why Beecher believed his medical readership—mostly non-Catholics, like Beecher himself<sup>117</sup>—would consider the pronouncements of a Roman Catholic pope morally authoritative, but asserting papal moral authority was apparently the best moral argument Beecher could think to offer.

Seeking to head off the sort of post-Brook Lodge anti-whistleblower blowback that he had received from his Harvard colleagues, Beecher reassures fellow medical professionals "that American medicine is sound, and most progress in it soundly attained. There is, however, a reason for concern in certain areas, and I believe the type of activities to be mentioned [i.e., unethical experiments] will do great harm to medicine unless soon

corrected. It will certainly be charged that any mention of these matters does a disservice to medicine, but not one so great, I believe, as a continuation of the practices to be cited.”<sup>118</sup> Thus, Beecher asserts here—as he did in his letters to Pappworth—that he is revealing unethical research not to blame or shame miscreant physicians or the journals that published their research; to the contrary, he sought to do the opposite, to protect the medical profession and medical research from “a great harm” by revealing a problem that requires correction.

In the next section, “Reasons for Urgency of Study,” Beecher provides data on the expansion of medical research, using the figures he had shared with Pappworth in his January 7, 1965, letter. He then details his generational change explanation of the increase in unethical experimentation observing that because “medical schools and university hospitals are increasingly dominated by investigators; Every *young man* knows that he will never be promoted to a tenure post, to a professorship in a major medical school, unless he has proved himself as an investigator. If the ready availability of money for conducting research is added to this fact, one can see how great the pressures are on ambitious *young physicians*.”<sup>119</sup>

Focusing on “the problem of consent,” Beecher next cites his *JAMA* article on “consent . . . myth and reality”<sup>120</sup> as the source of his claim that “consent in any fully informed sense may not be obtainable [from patients although it should] remain a goal toward which one must strive for sociologic, ethical, and clear-cut legal reasons.” The reason why consent should be viewed as a symbolic act and legal formality, rather than an effective moral safeguard, is that “if suitably approached, patients will accede, on the basis of trust, to about any request their physician may make.” Given the inadequacy of consent as an effective safeguard against unethical experiments, Beecher concludes, “A far more dependable safeguard than consent is the presence of a truly *responsible* investigator.”<sup>121</sup> Beecher then reemphasizes that the twenty-two cases he presents are not cited to condemn the investigators conducting the research: “they are record[ed] to call attention to a variety of ethical problems found in experimental medicine, for it is hoped that calling attention to them will help to correct abuses present.” Beecher then reiterates his standard refrain “that thoughtlessness and carelessness, not a willful disregard of the patient’s rights, account for most of the cases encountered. Nonetheless, it is evident that in many of the examples presented, the investigators have risked the health or the life of their subjects.” Thus, he claims, although the investigators may be guilty of thoughtlessly or carelessly endangering patients’ lives or health, they are not guilty of “willfully disregarding patients’ rights.” Consequently,

“references to the examples presented are not given, for there is no intention of pointing to individuals, but rather, a wish to call attention to widespread practices.”<sup>122</sup>

After summarizing the twenty-two cases, Beecher concludes by reprising his view that, of the two standard “safeguards” against the abuse of patients as human guinea pigs, “the more reliable safeguard is provided by the presence of an intelligent, informed conscientious, compassionate, responsible investigator,” rather than through the process of consenting the patient. He follows this claim by reiterating his utilitarian constraint on the ethics of research, stating that no experiment is ethical unless “the gain anticipated from an experiment [is] commensurate with the risk involved.” He then reinforces this utilitarian constraint with a somewhat neo-Kantian provision that “an experiment is ethical or not at its inception; it does not become ethical post hoc—ends do not justify means”; he finally concludes with a bit of Fletcher’s situation ethics, “There is no ethical distinction between ends and means.”<sup>123</sup>

Since Beecher stated these ideas in the decade before he submitted his paper to Garland, this hodgepodge of ideas are clearly his, not his editor’s. The paper ends with a comment about a professional initiative that he had mentioned in his January 25 letter to Pappworth: “In the publication of experimental results it must be made unmistakably clear that the proprieties have been observed. It is debatable whether data obtained unethically should be published even with stern editorial comment.”<sup>124</sup> This idea flows naturally from Beecher’s belief that the quest for publication is the source of researcher immorality, but it is tacked onto the paper with no analysis. It is just part of the potpourri of ideas running through Beecher’s mind inspired by his newly woke awareness of unethical research. Consistency may be the hobgoblin of little minds, but inconsistency is no bar to influence. Beecher’s article remains the most frequently cited journal article in the bioethics literature.<sup>125</sup>

Among the editorial challenges that impeded publication of Beecher’s article was the fact that lists make dull reading. Garland addressed this by more than halving Beecher’s list. Any publishing house contemplating publication of Pappworth’s manuscript would confront the exponentially greater challenge of transforming a list of 500 cases into a persuasive narrative. The first publishing house attempting to publish the manuscript was Victor Gollancz Ltd., which, in October 1962, issued Pappworth a contract for a manuscript of between 60,000 and 70,000 words due March 1963. As is not uncommon, Pappworth submitted the manuscript well after the deadline. Following standard practice, the manuscript was

submitted to reviewers. Their comments were uniformly negative. One wrote that “Dr. Pappworth’s . . . argument [is] extremely disjointed. The writing is often very muddled. . . . On the case histories . . . the verdict is that that they become terribly monotonous in their sameness, and so readability vanishes.” Another reviewer came to the same conclusion: the list of cases was “virtually unreadable” and would be “so to the great majority of laymen.” This report concludes, “It is not at all a *persuasive* book, and it becomes dull as a result of repetition.” After receiving the readers’ reports, in July 1963, Victor Gollancz wrote to Pappworth, “I am sorry to have to tell that we cannot publish ‘Human Guinea Pigs’ in anything like its present form. The real point is, to put it bluntly, it just isn’t in its present form, a book.”<sup>126</sup> After this, Pappworth sent his manuscript to three more publishers, who rejected it for the same reasons.

It was during this frustrating period, when his manuscript was receiving repeated rejections, that Pappworth wrote to Beecher about naming names and publicly blaming researchers and institutions, apparently to make his manuscript more newsworthy. “What would your reaction be to a suggestion,” Pappworth wrote, if “summaries of some of the most offensive experiments be published together with the journal references and names?”<sup>127</sup> In a later, February 20, 1965, letter, Pappworth informed Beecher that “after much thought and great hesitation I have decided that I shall publish a book on Human Guinea Pigs with quotations, names and references to about 150 of the 500-odd questionable experiments of which I have details.”<sup>128</sup> Beecher did not reply. Pappworth, however, reacted to Beecher’s June 1966 paper, “Ethics and Clinical Research,” by demanding to know why Beecher protected “the guilty” by not revealing their names. At the same time, Pappworth requested Beecher’s references for the twenty-two cases of unethical research cited in that article. Beecher replied that he and his editor had agreed that anonymity was the preferable format since the intent of the paper was to reveal a widespread practice, not to subject fellow physicians to potential criminal prosecution.<sup>129</sup> He also explained, “My attitude about what you call the ‘guilty’ is not one of protection for them alone. I did not want to detract from my object of pointing out a widespread practice, not individuals.”<sup>130</sup> Beecher later told Pappworth, “I am afraid I cannot send you further references. Everybody and his brother have been after me to divulge these names . . . I decided that the only sensible way was to decline in all cases to give information not contained in the paper itself.”<sup>131</sup> After this letter, collaboration between the two moralists came to a halt.

Around the same time, the summer of 1966, *Human Guinea Pigs* found a willing publisher, Routledge and Kegan Paul. Recognizing the challenges

presented by Pappworth's manuscript, the publishing house commissioned a freelance editor, Kathleen Orr, to work with Pappworth on editing the book. When the book came out in 1967, it drew the attention of such internationally renowned reviewers as the moral philosopher Sir Bernard Williams (1929–2003) and well-known author Arthur Koestler, CBE (1905–1983). It also commanded headlines in newspapers from tabloids to broadsheets. The *Daily Express* screamed, "What are your chances of being a human guinea pig?"; the *Daily Mail* stated more sedately, "Children, pregnant women, mental defectives, and old people were being used in National Health hospitals as 'guinea pigs' without consent . . . [Pappworth] likens some of the experiments to research carried out in the Nazi concentration camps."<sup>132</sup> A review in the more staid and scholarly *Economist* reported that "on the evidence scrupulously documented in this book, some practitioners of experimental medicine are wolves in white coats and are prepared to do hair-raising things to people admitted to a hospital's care . . . [Pappworth] is out to give a much-needed shock and will not be popular in medical high places; but he is right to insist that scientific enthusiasm should be tempered by humanity and prudence, and that the patient's interests should at all times be paramount."<sup>133</sup> On the other side of the Atlantic, noted cardiologist and author Michael Halberstam (1932–1980) wrote in the *New York Times* that Pappworth documented that "too often research physicians lose sight of the particular patient. . . . No physician who reads this book will ever make that mistake again and we shall all be better for it."<sup>134</sup>

Taking the opposite position, British gastroenterologist and award-winning researcher, Sir Christopher Booth (1924–2012), a doyen of the British medical establishment who later became president of the British Medical Association (BMA) and the Royal College of Physicians (RCP founded 1518), challenged Pappworth's claims. "It is palpably untrue to claim that the majority of doctors are either genuinely ignorant of the immensity and complexity of the problem [of unethical experiments on patients] or wish purposely to ignore the whole matter by sweeping it under the carpet . . . [Pappworth's] work treats the serious subject of the involvement of human beings in research without sufficient understanding, personal experience, or knowledge." Booth concludes that Pappworth "is to be condemned for his continued attacks on his professional colleagues who, by their research, are doing all they can to improve the treatment they give to sick people who come to them for help."<sup>135</sup> In this ad hominem critique, Booth denies Pappworth's standing as an expert, claiming that he lacks "sufficient understanding, personal experience, or knowledge" and



goes on to condemn Pappworth as a traitor to fellow medical professionals for “his continued attacks on his professional colleagues.”

The blowback against Pappworth differs markedly from that directed against Beecher. In part, this was because Pappworth, smarting from a lifetime of anti-Semitic rejections, was “not known as a ‘man to mince words.’”<sup>136</sup> Pappworth’s blunt, often acerbic style was paid back in kind by reviewers. In *The Lancet*, Pappworth’s cases were dismissed as a “vitriolic amplification of some of the charges [that Pappworth has been] firing at the medical profession for years, especially in his cannonade of a book.” “Shorn of such excesses and some of its haughtiness his book would have a greater impact on those he is presumably trying to influence.”<sup>137</sup> Yet Pappworth believed that professional status should not shield unethical experimenters from public accountability, so he named and publicly shamed prominent professionals, powerful medical institutions, and distinguished journals. When eminent, prominent, and powerful professionals or institutions attempted to shush him, he replied, “Those who dirty the linen and not those who wash it should be criticized. Some do not wash dirty linen in public or private and the dirt is left to accumulate until it stinks.”<sup>138</sup> As physician-historian Paul J. Edelson observes,

Pappworth was personally attacked—he was called “shrill,” his work was described as “slanted,” he was said to lack the “restraint” necessary to write a “more effective” book—in ways that Beecher, always referred to, in print, as “Professor Beecher” or as *Time Magazine* put it “Harvard’s Dr. Beecher,” as Pappworth never was. And, unlike Beecher, whose article has been widely cited and reprinted at a crucial moment in the debate over medical experimentation, [by 2004] Pappworth’s book had been cited in the medical literature fewer than a dozen times since its appearance in 1967. . . . Publicly [Pappworth] was repeatedly dismissed as a troublemaker and, it may be as a result, he was not considered an acceptable person to be referred to publicly at meetings or professional discussions regardless of the substance of his remarks.<sup>139</sup>

It should be noted that just over a half-century later, at the end of 2021, Pappworth’s effort as a pioneering bioethicist has been more widely recognized, and *Human Guinea Pigs* has been cited over 500 times.<sup>140</sup>

“At the core of [Pappworth’s] story,” observed Professor Martin Gore, CBE, FRCP (1951–2019), “is the clash between a little Jew brought up in poor circumstances, who came from [provincial] Liverpool rather than [cosmopolitan] London, had none of the requisite charms or behaviors apparently required, was revered by his students but dismissed by a powerful establishment of academics who know everything but understood nothing.” “Pappworth’s life story [was also about] the anti-Semitism Pappworth

undoubtedly encountered as a medical student, during the war, and in the 1950s and 60s.”<sup>141</sup> In contrast, as Edelson observes, “Beecher . . . was generally treated respectfully in public, which, perhaps, allowed people to use him both substantively and symbolically as a sponsor of medical reforms in which they were interested.”<sup>142</sup> Another factor was that Pappworth, having little faith in the profession that rejected him as an uppity Jew, publicly identified, named, and shamed miscreant researchers, institutions, and journals, whereas Beecher declined to do so. Just as importantly, whereas Pappworth looked to external bodies outside the profession to prevent unethical research (that is, to regulation and regulatory bodies); Beecher, in contrast, believed that the profession should reform itself by acting without recourse to externally imposed regulations or laws. Thus, he recommended that professional journals require that researchers document how they had safeguarded their patients interests and rights as a prerequisite to publication. He also urged journal editors refuse to publish morally dubious experiments. Beecher’s focus on professional self-reform made his proposals less threatening to fellow professionals. This may be why his collaboration with Pappworth ceased after Pappworth asked for the names and journal references for the twenty-two cases that Beecher described in “Ethics and Clinical Research.” It was more than a difference in methodology whether to focus on moral reform or on punishing immoral researchers; another factor was that Beecher was a former outsider who now proudly held a named professorship at Harvard. Having become an eminent insider, he sought to avoid returning to his outsider status. Pappworth, forever an outsider, had nothing to lose—and notoriety and free publicity to gain—by naming and shaming.

Curiously, with a few minor changes, Pappworth’s story might have been Beecher’s as well. At the core of Harry Unangst’s story is the clash between his youth as a bible-reading born-again full-immersion Congregational Methodist who, like Pappworth, was brought up in poor circumstances in a provincial town, Peck, Kansas, rather than in a cosmopolitan city, like Boston or London. Unlike Pappworth, however, Beecher, having the requisite charms and behaviors, acculturated successfully by assuming the persona of the Boston Brahmin, Henry Knowles Beecher. He also married up in social status and became a doyen of the Boston and Harvard establishments. As Edelson observes, status accounts in large measure for how differently Beecher and Pappworth were treated by their professions, and it helps to explain why Beecher’s paper achieved its now iconic status as a classic of medical ethics and why both Harvard and the prestigious bioethics thinktank, the Hastings Center, offered Henry K. Beecher awards

in medical ethics.<sup>143</sup> Pappworth's book, in contrast, has the secondary status of a whistleblowing tract and, as of 2022, no professional society or ethics center offers a Pappworth award. To the contrary, the profession treated Pappworth as a pariah, denying him the title of Fellow of the Royal College of Physicians for an unheard-of fifty-seven years. He finally received the honor a few months before his death and then only after a fellow East European Jew from Manchester, Professor Leslie Turnberg (later Barron Turnberg), became the president of the Royal College of Physicians. At age of eighty-three, Pappworth, the brash whistleblowing Manchester Jew, was, for a few months before his death, accepted as a Fellow of the Royal College of Physicians (FRCP).



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**How African Americans, Anti-Nazis, Bureaucrats,  
Feminists, Veterans, and Whistleblowing  
Moralists Created Bioethics**

**By: Robert Baker**

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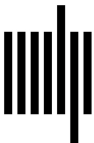
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