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What Does It Mean to Go Public? The American Response to Lysenkoism, Reconsidered

ABSTRACT

The American response to Lysenkoism took place at a crucial moment in the evolving relationship between science and the public. Like many professional scientific organizations in the early Cold War, the Genetics Society of America (GSA) resisted involvement in political issues. In contrast to similar societies in the physical sciences, however, the geneticists' silence cannot be explained solely by the fear of financial or political repercussions. Rather, the GSA's reluctance to engage in political discussion reflected an ongoing debate within the scientific community on the proper role for professional societies in political controversy. Those geneticists who did become embroiled in the controversy did so as individuals rather than as emissaries of the profession. Geneticists H. J. Muller, L. C. Dunn, and Theodosius Dobzhansky attempted to reach the public through a variety of outlets, including books, magazines, newspapers, and the radio, but their interventions were shaped by their individual personal and political commitments. The GSA, in contrast, attempted to combat the spread of Lysenkoism with the help of a public relations firm and a Golden Jubilee celebration of the rediscovery of Mendel's laws. The messy story of the American response to the Lysenko crisis demonstrates the limits of scientists' political involvement during the early Cold War.

KEY WORDS: Cold War, Trofim Lysenko, genetics, scientific activism, science and politics, Mendel, commemoration, science and the media

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The following abbreviations are used: AAAS, American Association for the Advancement of Science; AIBS, American Institute of Biological Sciences; APS, American Philosophical Society, Philadelphia, PA; ASHG, American Society for Human Genetics; GSA, Genetics Society of America Papers, American Philosophical Society, Philadelphia, PA (note: in the text, GSA is used to simply refer to the Genetics Society of America); HJM, H. J. Muller Papers, Correspondence Series, Lilly Library, Indiana University, Bloomington, IN; *JHB*, *Journal of the History of Biology*; LCD, L. C. Dunn Papers, American Philosophical Society, Philadelphia, PA; *SRL*, *Saturday Review of Literature*; TD, T. Dobzhansky Papers, American Philosophical Society, Philadelphia, PA.

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INTRODUCTION

The story of the rise to power of the Soviet geneticist Trofim Lysenko (1898–1976) is perhaps one of the best-known cautionary tales in the history of science. Together with Galileo’s heresy, the Scopes trial, and—a recent addition—the ongoing debate on climate change, the Lysenko affair represents the triumph, albeit temporary, of political authority over scientific reason. Historians’ analyses of the incident have yielded keen insights on a range of issues, from diplomatic relations between the United States and the Soviet Union, to the internal operations of the *politburo*, to the development of twentieth-century biological thought.¹ But if the story of Lysenkoism within the Soviet Union is by now a familiar one, the response of the American scientific community to Lysenkoism deserves another look.

Coming at the dawn of the postwar era, the American response to Lysenkoism marks a crucial turning point in scientists’ attitudes about the proper relationship between science and the public. Over the course of five years, a core group of influential American geneticists debated how, and whether, they as scientists should engage in what was essentially a political issue. These scientists’ actions reflect a transition that was taking place throughout the scientific community, as physicists, chemists, and biologists gradually became more reluctant to be seen as public figures. Physical scientists could point to government secrecy and security clearances as the source of their sudden silence, but the

1. Most early histories of the Lysenko affair, some written by participants, proclaim the view that ideology had replaced rational science. The classic early accounts are Z. A. Medvedev, *The Rise and Fall of T. D. Lysenko*, trans. I. Michael Lerner (New York: Anchor Books, 1971); and David Joravsky, *The Lysenko Affair* (Cambridge, MA: Harvard University Press, 1970). Although written after the end of the Cold War, Valery N. Soyfer, *Lysenko and the Tragedy of Soviet Science*, trans. Leo Gruliov and Rebecca Gruliov (New Brunswick, NJ: Rutgers University Press, 1994) takes a similar point of view. In addition, see Mark B. Adams, “Genetics and the Soviet Scientific Community, 1948–1965” (PhD dissertation, Harvard University, 1972); Loren Graham, *Science and Philosophy in the Soviet Union* (New York: Knopf, 1972); and Nikolai Krementsov, *Stalinist Science* (Princeton, NJ: Princeton University Press, 1997). As to the question of whether there was room for genuine scientific disagreement with regard to Lysenko’s theories, see Nils Roll-Hansen, *The Lysenko Effect: The Politics of Science* (Amherst, NY: Humanity Books, 2005). Specific discussions of the American geneticists’ campaign can be found in Krementsov, “A ‘Second Front’ in Soviet Genetics: The International Dimension of the Lysenko Controversy, 1944–1947,” *JHB* 29 (1996): 226–50, and Jan Sapp, *Beyond the Gene: Cytoplasmic Inheritance and the Struggle for Authority in Genetics* (New York: Oxford University Press, 1987), 168–80. For discussions of the British response, see Oren Solomon Harman, “C. D. Darlington and the British and American Reaction to Lysenko and the Soviet Conception of Science,” *JHB* 36 (2003): 309–52; and Diane Paul, “A War on Two Fronts: J. B. S. Haldane and the Response to Lysenkoism in Britain,” *JHB* 16 (1983): 1–37.

choice to turn away from public dialogue was (at least slightly) less coercive for biologists.² By 1950, disillusioned by an unpredictable public reception and roadblocks within their professional organizations, many biologists were losing their faith in the ability of individual scientists to effect change in public opinion. The narrative arc of their experience mirrors a broader cultural and political debate about the role of science in civil society.

The Lysenko episode reached its climax at a particularly delicate moment in the relationship between science and democracy. Scientists, as the producers and keepers of the knowledge that could produce radar or an atomic bomb, both provided the legitimacy for postwar American foreign policy supremacy and posed a threat to traditional structures of liberal democracy. Scientists seemingly held the key to knowledge that elected officials could neither produce nor understand.³ Simultaneously, as political theorist Yaron Ezrahi has argued, the depersonalized, objective conception of science that dominated this era provided a means of establishing the depersonalized authority of large bureaucratic structures within the federal government.⁴ These concrete, material aspects of the scientific community's relationship to power and authority should not be dismissed.

At the same time, however, a portion of the global scientific community was engaged in a more abstract discussion about the relationship between science and democracy. Unlike the governmental advisors who concerned themselves with the question of how to maintain some semblance of a liberal democracy

2. Obviously, there were many individual exceptions to the collective scientific turn away from true public engagement during the postwar period. My analysis owes much to Jessica Wang's discussions of the actions of physical scientists in the immediate postwar years. See Jessica Wang, *American Science in an Age of Anxiety: Scientists, Anticommunism, and the Cold War* (Chapel Hill: University of North Carolina Press, 1999); and Jessica Wang, "Scientists and the Problem of the Public in Cold War America," *Osiris*, 2nd ser., 17 (2002): 323–47. Kelly Moore's *Disrupting Science: Social Movements, American Scientists, and the Politics of the Military, 1945–1975* (Princeton, NJ: Princeton University Press, 2008) offers counterexamples of scientists who chose to engage the public in ongoing discussions about science and militarism throughout the 1950s, 1960s, and 1970s.

3. This basic point has become commonplace in analyses of Cold War science. See especially Paul Boyer, *By the Bomb's Early Light: American Thought and Culture at the Dawn of the Atomic Age* (New York: Pantheon Books, 1985); Gregg Herken, *Cardinal Choices: Presidential Science Advising from the Atomic Bomb to SDI*, rev. ed. (Palo Alto, CA: Stanford University Press, 2000); Daniel J. Kevles, *The Physicists: The History of a Scientific Community in Modern America* (New York: Vintage, 1979); Charles Thorpe, *Oppenheimer: The Tragic Intellect* (Chicago: University of Chicago Press, 2006); and Wang, *American Science* (ref. 2).

4. Yaron Ezrahi, *The Descent of Icarus: Science and the Transformation of Contemporary Democracy* (Cambridge, MA: Harvard University Press, 1990).

in a technocratic state, these scientists were engaged with the more theoretical question of whether the practice of science itself could provide a model for democratic society. David Hollinger has demonstrated the deep commitment of scientists on the progressive left to a notion of the “scientific ethos” that stressed the egalitarianism and high moral standards of the scientific community. Commentators as varied as the philosopher Sidney Hook, the biologist C. H. Waddington, and, most famously, the sociologist Robert Merton, all agreed that there was some sort of natural connection—however ill-defined—between the scientific enterprise and democratic political culture.⁵

In recent years a number of scholars have turned their attention to the tension between these ideals of an enlightened, authoritative scientific community and a Cold War culture that was increasingly suspicious of individual political motives and actions. In Charles Thorpe’s biography of J. Robert Oppenheimer, for example, we see how the Manhattan Project scientist attempted to construct an identity based as much on his personal charisma as on his scientific mettle, and how this strategy ultimately soured in the environment of heightened suspicion that characterized the postwar political climate.⁶ In Jessica Wang’s account of the atomic scientists’ thwarted efforts to take their message of peaceful uses of atomic energy to schoolchildren, Kiwanis Clubs, and church groups, we see the harsh glare of the surveillance state monitoring the public relations efforts of scientific professionals.⁷ Elsewhere in Wang’s work and in that of sociologist Kelly Moore, we see the growing reluctance of professional scientific societies to participate in civil discourse, whether out of fear of investigation, reluctance to alienate potential allies or funding agencies, or, in some cases, personal aversion to controversy among the societies’ elected officers.⁸

Like much of the scholarship on science and the Cold War, these recent studies have mostly focused on the actions of physical scientists, whose interventions into matters of public policy were on issues whose importance was self-evident to the state. But what of the biologists? Aside from the teachings of a few radicals, Lysenkoism had few supporters on American shores. In the face of what seemed to be growing evidence of executions and disappearances

5. David A. Hollinger, “The Defense of Democracy and Robert K. Merton’s Formulation of the Scientific Ethos,” *Knowledge and Society* 4 (1983): 1–15.

6. Thorpe, *Oppenheimer* (ref. 3); and Charles Thorpe, “Disciplining Experts: Scientific Authority and Liberal Democracy in the Oppenheimer Case,” *Social Studies of Science* 32 (2002): 525–62.

7. Wang, “Problem of the Public” (ref. 2).

8. Wang, *American Science* (ref. 2); Moore, *Disrupting Science* (ref. 2).

among their colleagues in the Soviet Union, opposition to Lysenkoism—and the political structures that enabled it—should have been an issue around which even the most consensus-minded geneticist could rally. With its elements of human drama and political intrigue, the issue seemed tailor-made to attract media coverage. Yet, for a variety of reasons, the American genetics community as a whole chose largely to stand on the sidelines. Those few scientists who attempted to rouse a sense of outrage among their colleagues in the Genetics Society of America (GSA) were met with silence or polite demurrals. Those geneticists who did attempt a campaign against Lysenkoism turned to the popular press, not scientific journals, and they hoped to attract attention to their cause without mentioning the Communist Party. Eventually, concerned about the public actions of rogue elements insistent on action, the GSA turned to a public relations firm to highlight the achievements of Western genetics as part of a plan to condemn Lysenko without even mentioning his name.

This episode raises important questions about the limits of scientists' roles as public figures, both as individuals and as members of scientific institutions. Beyond its importance to the history of science in the Cold War, it provides insight into the mechanisms of public relations during a time when the very notion of "science and the public" was changing. What did scientists hope to accomplish by bringing their argument directly to the public, in the form of mass media, rather than to their scientific peers? Was the public merely to listen and acquiesce, or were disagreements welcome? How are we to understand the geneticists' refusal to condemn Lysenko publicly, when their private correspondence makes clear he was their central target? What were the terms under which scientists were expected to engage in controversial discussions, whether scientific or political?⁹

OPENING SALVOS: TRANSLATING HEREDITY AND ITS VARIABILITY

One might have expected Theodosius Dobzhansky to have spent July 4, 1945, celebrating Independence Day with his wife, Natasha, and their daughter, Sophia. Both Theodosius (Dodek to his friends) and Natasha had emigrated

9. The cultural authority of science and medicine is a concept that underlies most work by historians of American science and medicine today. For works that specifically address this issue, see Ronald G. Walters, ed., *Scientific Authority and Twentieth-Century America* (Baltimore: Johns Hopkins University Press, 1997); Charles E. Rosenberg, *No Other Gods: On Science and American Social Thought*, rev. ed. (Baltimore: Johns Hopkins University Press, 1997); Paul Starr, *The Social Transformation of American Medicine* (New York: Basic Books, 1982).

from the Ukraine to the United States in the 1920s and thoroughly enjoyed the more outlandish aspects of American culture. Summers involved cross-country drives from New York City to Mather, California, where Dobzhansky divided his time between horseback riding and collecting fruit flies in the wild.¹⁰ On this particular morning, he had driven all night from Berkeley to protect a load of flies from the heat of the San Joaquin Desert. This time the hiking and riding, however, were tempered by the mixed news he carried along with the flies: Lysenko's position seemed less secure, but Soviet geneticists Nikolai Vavilov, Georgi Karpechenko, and G. Levizky were almost certainly dead.¹¹

Genetics news from the Soviet Union—and indeed, much of Europe—had been sporadic throughout World War II. This much, however, was clear: Trofim Lysenko, an opponent of Western genetics (as he called it, “Mendelism-Morganism-Weismannism”), had taken charge of much of Soviet agricultural research. As director of the Lenin All-Union Academy of Agricultural Sciences, he headed twenty-four research institutes and a number of affiliates. Lysenko opposed the work of Western geneticists on the grounds that their theoretical investigations offered no practical applications and therefore directly interfered with the work of the people. He argued that his own scientific contributions, in contrast, held the potential to transform Soviet agricultural production. His most notorious theories included wheat vernalization, in which spring wheat was transformed into winter wheat through environmental “education,” and vegetative hybridization, in which hybrid offspring were produced through grafting.

Most Western geneticists dismissed these results as ridiculous. Lysenko himself could not be so easily dismissed. His position as director of the Agricultural Academy suggested the extent of his influence. More worrisome were his attempts to dismantle Western genetics within the Soviet Union. The international genetics community blamed Lysenko for the cancellation of the 1937 International Genetics Conference in Moscow and the absence of Soviet geneticists from the 1939 International Genetics Conference in Edinburgh. More worrisome still, Vavilov, a Soviet geneticist of international renown, had not been heard from since shortly after he opposed Lysenko in

10. Dobzhansky's frequent letters to L. C. Dunn provide much of the background information for this section. Their correspondence is divided between the Dunn Papers (LCD) and the Dobzhansky Papers (TD), both located at APS.

11. Dobzhansky to Dunn, 4 Jul 1945, LCD. Historians now believe that Vavilov died on January 23, 1943; Karpechenko on July 28, 1941; and Levizky on May 20, 1942. Soyfer, *Lysenko and the Tragedy* (ref. 1), 357–58.

a 1940 debate. Other prominent geneticists, including Karpechenko, were reported missing as well.

Many American geneticists had known and worked with these Soviet geneticists for many years and considered them friends. But at the same time that they mourned the fate of their colleagues, Dobzhansky and his Columbia University colleague Leslie C. Dunn believed that there might be cracks in Lysenko's grip on power and thought that the time for action was at hand. The situation was complicated, however, by their lack of knowledge of the situation on the ground. *If* their colleagues were still alive, Western diatribes against Lysenko might endanger their lives. Moreover, Dunn, like many members of the biological community in both the United States and Great Britain, was committed to leftist politics and was reluctant to be seen as criticizing the Communist Party.¹² Nevertheless, Dobzhansky and Dunn agreed that it would be better for support for Soviet geneticists to come from quarters sympathetic to Marxism, and Dunn somewhat reluctantly agreed to accept the mantle as an American spokesperson against Lysenkoism.

Dunn might have been willing to enter the battle, but his and Dobzhansky's weapon of choice seems—at least in retrospect—somewhat peculiar. Drawing on ideas of scientific objectivity and disinterestedness, Dunn and Dobzhansky believed that the best way to combat Lysenkoism was to let the Soviet scientist speak for himself. They therefore embarked on a translation of Lysenko's best-known work, *Heredity and Its Variability*. Dobzhansky acted as translator; Dunn attempted to secure a publisher for the work. Dobzhansky found translating Lysenko's work profoundly depressing, and his desire to avoid the topic had slowed the work's progress. In a letter to Dunn explaining his tardiness, he wrote, "Translating it has been one of the most unpleasant tasks I had in my whole life, and surely I would never undertake a thing like that for the money—it can be done only for a 'cause.'"¹³

12. For additional biographical information on Dunn, see Melinda Gormley, "Scientific Discrimination and the Activist Scientist: L. C. Dunn and the Professionalization of Genetics and Human Genetics in the United States," *JHB* 42 (2009): 33–72. Gormley skirts the question of whether Dunn was a member of the American Communist Party. In the late 1930s, Dunn was a member of the Executive Committee of the American Committee for Democracy and Intellectual Freedom, an organization accused of being a Communist front by the prominent liberal anti-Communist Sidney Hook in his memoir, *Out of Step: An Unquiet Life in the 20th Century* (New York: Harper and Row, 1987), 249. Gormley describes Dunn (using Dunn's own language) as a Fabian Socialist. His American contemporaries would have characterized him as a Popular Front liberal or liberal anti-Fascist.

13. Dobzhansky to Dunn, 31 Jul 1945, LCD.

Dunn attempted to interest Simon and Schuster in the manuscript, but the publisher turned him down, citing a limited market. When he pitched it to King's Crown Press, a division of Columbia University Press, Dunn stressed that while the Lysenko controversy had attracted much media attention, "chiefly in the political press," reporters and readers had been unable to evaluate Lysenko's actual scientific experiments. In Dunn's estimation, the translation's value lay in its ability to "place Lysenko's views directly before the American public so they may draw their own conclusions concerning his work."¹⁴ The two truly intended the volume to stand alone; Dobzhansky resisted Dunn's offers to write an introduction himself or solicit one from former Vice President and Secretary of Agriculture Henry Wallace.¹⁵ (Wallace was deeply interested in Lysenko's work and had expressed curiosity about its potential applications; he was, of course, also known as a friend of the left.) Dobzhansky's reluctance stemmed partially from his desire to keep Dunn's and Wallace's names from being associated with Lysenko's experiments, but it also reflected Dobzhansky's feeling that the facts should speak for themselves: "Let him stand on his own feet."¹⁶ The eventual volume included only a minimalist translator's preface in which Dobzhansky explained the reason for the undertaking and indicated that he objected to Lysenko's work. By the end of the summer, Dobzhansky's translation was complete, and King's Crown Press had agreed to publish 4,000 copies in pamphlet form for fifty cents each.¹⁷

At the same time that Dobzhansky and Dunn wanted the volume to stand alone, they realized that successful publication would require publicity—and even more important, the right kind of publicity. Although they seemed to be directing their energies toward the public at large by publishing the book in pamphlet form,

14. Dunn to Henry Silver, 17 Aug and 11 Nov 1945; Silver to Dunn, 11 Sep and 4 Oct 1945; all in LCD.

15. Dobzhansky to Dunn, 4 Sep 1945, LCD. Wallace, incidentally, had followed the controversy closely, and he sometimes sent Dunn copies of Soviet works on genetics upon his return from trips abroad. While Wallace mostly shared Dunn's perspective on Lysenko, he cautioned the geneticist to consider carefully the possible applications of Lysenko's work. Commenting on several Soviet works for which he had seen translations, Wallace wrote, "I can see from this that your assessment of Lysenko is quite accurate. He probably isn't a scientist at all, but he has a fresh point of view which may conceivably have in it a small residue of value. At any rate, I don't think we should dismiss him completely, even though his temperament is completely non-scientific." Wallace to Dunn, 12 Aug 1945, and 7 Feb 1946; Dunn to Wallace 31 Aug 1945, and 30 Jan 1946; and Dobzhansky to Dunn, 18 Aug 1945; all in LCD.

16. Dobzhansky to Dunn, 4 Sep 1945, LCD.

17. Dobzhansky to Dunn, 31 Jul 1945, LCD. The work was published as T. D. Lysenko, *Heredity and Its Variability*, trans. Th. Dobzhansky (New York: King's Crown Press, 1946).

they nevertheless were suspicious of the popular press. In December, Dunn learned that the Hearst Press newspaper empire planned to use the book as anti-Soviet propaganda. This was, of course, exactly the situation that Dunn had hoped to avoid. Greatly alarmed, Dunn urged fellow leftist scientists and science writers to review the book from a purely scientific perspective. In his request for Lewis J. Stadler to review the book in *American Naturalist*, for example, Dunn wrote, “It is very important that the book be reviewed by persons who understand the implications of his work and who will not use it merely for an attack on Soviet science in general. The book was translated in order that Americans could judge first hand what Lysenko’s ideas really are. It is clearly anti-scientific and should be so criticized regardless of the author’s nationality.”¹⁸ The well-known geneticist H. J. Muller, a former Communist, made a similar appeal on Dunn and Dobzhansky’s behalf to J. B. S. Haldane, the prominent Communist British biologist who had not yet given up on Lysenkoism.¹⁹ Dunn himself wrote the review for *Science*; Dobzhansky for the *Journal of Heredity*; and their colleague G. Ledyard Stebbins for the *Chronica Botanica*.²⁰ Dunn also sent copies of these reviews to Waldemar Kaempffert, science editor of the *New York Times*, in response to Kaempffert’s request for information on the controversy.²¹

Their pleas for scientific coverage met with mixed success. Neither Haldane nor Stadler agreed to lend their pens. While Haldane somewhat disingenuously simply pleaded unfamiliarity with the book, Stadler explained that he missed Dunn’s point: “I cannot find anything to say about the Lysenko job that seems to me worth printing. . . . It is a useful example of unscientific method, and I think I shall want a few copies to give to graduate students.” But as a lesson in objectivity and open-mindedness, he said he could think of many “indigenous examples” and could “see no advantage to Russian-American relations in choosing one from Russia.”²²

18. Dunn to L. J. Stadler, 22 Nov 1945, LCD, Folder Lysenko Controversy in the U.S. #2.

19. Muller et al. to J. B. S. Haldane, 7 Apr 1946, HJM. For an insightful discussion of Haldane’s position on Lysenkoism vis-à-vis his role in the Communist Party, see Paul, “War on Two Fronts” (ref. 1).

20. L. C. Dunn, “Review of *Heredity and Its Variability*,” *Science* 103 (1946): 180–81; Theodosius Dobzhansky, “Review of *Heredity and Its Variability*,” *Journal of Heredity* 37 (1946): 5–9. Oddly, no one seems to have objected to the obvious conflict of interest of having the translator and sponsor of the book write its reviews.

21. Waldemar Kaempffert to Dunn, 29 Jan 1946; and 1 Feb 1946; Dunn to Kaempffert, 31 Jan 1946; all in LCD, Folder Lysenko Controversy in the U.S. #2.

22. Stadler to Dunn, 28 Nov 1945, LCD, Folder Lysenko Controversy in the U.S. #2. The State Department refused to grant Stadler a passport to attend the International Congress of

Stadler's comments highlight the more problematic aspects of Dunn's plan. At the same time that Dunn and Dobzhansky wanted to bring media attention to the translation, they feared that any discussion of the book's political context might either exacerbate the problems faced by Soviet scientists or inflame the already tense diplomatic relationship between the United States and the Soviet government. Perhaps more to the point, dedicated Communists such as Haldane were uncomfortable airing the party's dirty laundry.²³

If Dunn had initially hoped that readers would approach the translation in the same objective spirit that scientists supposedly approached their experiments, he was beginning to realize that a more sophisticated strategy might be necessary. When corresponding with the scientific press, he maintained the position that his sympathy for the Soviet cause proved his objectivity as a reviewer. In his cover letter to his unsolicited *Science* review, for example, Dunn wrote, "Because of my position and known sympathy with the development of science in the U.S.S.R., such criticism, as contained in the review, cannot be attributed to animosity or prejudice but merely to a desire to judge Russian scientific work by the same standards by which other scientific work is judged."²⁴ When presenting the issue to Kaempffert at the *New York Times*, however, Dunn explicitly acknowledged that his objectivity was itself a strategy:

You will probably want to refer to the motives which American geneticists had in bringing out Lysenko's book in English. There are two points of view here. Some American scientists thought it better not to dignify Lysenko's rather vague and mystical ideas by serious treatment and criticism. The other point of view rejects this as not conforming to the usual method of science which insists that what is criticized must be thoroughly understood first. Since I belong to this latter group, I believe that objective discussion of the scientific and practical bases of Lysenko's theories will eventually be a worthy service to Soviet science. The fact that it may be used as a whip by those who wish to abuse the Soviet Union I think matters less, since it will have a temporary effect.²⁵

Genetics in Stockholm in 1947 on the basis of his involvement with the American Committee to Save Refugees. If Stadler really was a Communist, he may not have felt free to criticize Lysenko's views in a public forum. For more on the Stadler controversy, see the Stadler folder in the Correspondence Series, HJM. For Haldane's terse refusal, see Haldane to H. J. Muller et al., 15 May 1946, HJM.

23. Haldane's eventual renunciation of Lysenko and the Communist Party in 1950 came much too late to be of use to Soviet geneticists. See Paul, "War on Two Fronts" (ref. 1).

24. L. C. Dunn to the Editor of *Science*, 15 Jan 1946, LCD, Folder Lysenko Controversy in the U.S. #2.

25. Dunn to Kaempffert, 31 Jan 1946, LCD, Folder Lysenko Controversy in the U.S. #2.

Dunn's explanation to Kaempffert is slightly disingenuous. He does not mention, for example, that he is one of only two American geneticists who brought about the book's publication. And while true that the volume was published in the interest of understanding Lysenko's work and not merely as an act of criticism, Dunn's correspondence with Dobzhansky makes clear that publication was also meant to aid in Lysenko's annihilation. Even so, Dunn's broader point about divergent strategies within the American genetics community rings true. While Dunn, Dobzhansky, and Muller formed the core of a group who hoped to use publicity to ameliorate the crisis in Soviet genetics, a large contingent of the GSA argued that ignoring Lysenko offered the best hope for his destruction.

From this perspective, Dunn's and Dobzhansky's attempts to publicize Lysenko's works can be considered a fundamentally radical act. Rather than simply counter bad science with good science, they acknowledged that it might take more than the scientific method to achieve public support for their goals. Yet Dunn's position was complicated by his remaining political allegiances. He was infuriated, for example, when he read Robert Simpson's review of the translation in the March 1946 issue of the *Saturday Review of Literature*.²⁶ Simpson made the cardinal error, from Dunn's perspective, of claiming "that Lysenko is typical of science in the Soviet Union." Dunn's three-page letter to the editor seethed that "one should no more view the whole of Russian science through the lens of Lysenko, than one should view American science through fundamentalist writings on evolution."²⁷ Dunn's public response to Simpson's review exposed him to additional criticism, and Simpson took the bait: Simpson's response implied that Dunn's comparison of Soviet and American scientific politics amounted to an apologetic defense of "Russian totalitarian politicians."²⁸ The political atmosphere of 1946, with growing tensions between Communism and democracy, held no patience for Dunn's careful distinctions between Lysenkoism and the rest of Soviet science.

Dunn's and Dobzhansky's publishing strategy was based on scientific ideals of open-mindedness and objectivity. They drew the line at actually condemning either Lysenko's scientific theories or his political power, hoping instead that an informed general public would draw its own conclusions. But by 1948 any

26. Robert Simpson, "Science, Totalitarian Model," *SRL*, 9 Mar 1946, 28–32.

27. L. C. Dunn to the Editor of the *SRL*, 11 Mar 1946, LCD, Folder Lysenko Controversy in the U.S. #2. Dunn's letter was published in the March 30, 1946 issue.

28. Robert Simpson, "Response to L. C. Dunn's letter to the Editor," *SRL*, 30 Mar 1946, 29–30.

hope that Western scientific criticisms of Lysenko's science might reduce his power was shattered. In August of that year, Lysenko announced that his theories had received the official endorsement of the Communist Party.²⁹ Within a month's time, the Soviet Academy of Sciences had removed the remaining leaders of Soviet genetics from their posts at leading research institutes.³⁰ Lysenkoism had become a political, not a scientific, problem.

TO THE BARRICADES: BROADSIDES AND DIATRIBES

The news of August 1948 galvanized the genetics community. Dobzhansky, upon hearing it while on sabbatical in Brazil, moaned, "I am lost to invent what to do, especially while sitting here, so far from the center of things!" At this point, he felt Western geneticists had no choice but condemnation: "If we do not speak out then stones shall speak!" He dashed off letters to his colleagues, asking whether the American Association for the Advancement of Science (AAAS), or the National Academies, or the GSA, or *someone* might make some sort of statement.³¹

Muller, newly minted as a Nobel Laureate, wholeheartedly agreed and urged Dobzhansky to draft a statement to be signed by himself, as president of the American Society for Human Genetics (ASHG), and L. H. Snyder, the president of the GSA, the two most important professional organizations for American geneticists.³² But, like Dunn, Muller had his own complications. Muller had spent several years in the 1930s working in Leningrad and Moscow, and he had published a radical newspaper, the *Spark*, while a young faculty member at the University of Texas, Austin. Life in the Soviet Union proved a disappointment

29. Kremontsov has examined the American geneticists' 1946 campaign in light of Soviet Westernization. He suggests that Lysenko's triumph in 1948 had more to do with Soviet isolationism and nationalism than any true belief in Lysenko's theories. Kremontsov, "Second Front" (ref. 1).

30. For good documentary histories of these events, see "The History of the Genetics Conflict," *Bulletin of the Atomic Scientists* 5, no. 5 (1949): 131–40; and Conway Zirkle, ed., *Death of a Science in Russia: The Fate of Genetics as Described in Pravda and Elsewhere* (Philadelphia: University of Pennsylvania Press, 1949).

31. All of the above quotes are from Dobzhansky to Dunn, 9 Sep 1948, LCD, Folder Dobzhansky Papers, 1948–1949. Dobzhansky mailed a nearly identical letter to Muller on the same day but not coincidentally left out a comment about red-baiting. See Dobzhansky to Muller, 9 Sep 1948, HJM.

32. Muller to Dobzhansky, 27 Sep 1948, HJM.

to Muller, and Lysenko's rise to power in the late 1930s only exacerbated Muller's unhappiness with the realities of Stalinism. He and his wife, Thea, fled to Edinburgh in 1937 after Stalin reacted badly to a copy of Muller's eugenic tract, *Out of the Night*, that Muller had sent him. By the end of World War II, Muller had become something of a red-baiter, only one whose peculiar brand of politics included eugenic socialism as well as rabid anti-Communism. He was frequently invoked by or appeared on committees with such outspoken liberal anti-Communists as John Dewey, James T. Farrell, and Sidney Hook.³³ Unlike Dobzhansky's and Dunn's earlier attempts to present the faults of Lysenko's science, Muller threw himself directly into political controversy. Muller recognized the potential of a public campaign to both reshape his own reputation and fight Joseph Stalin's excesses. Needless to say, his political views were shared by few members of the general population, and his approach to the Lysenko controversy alarmed some members of the GSA.

Muller wasted no time in taking individual action. On September 24, 1948, Muller publicly resigned from the Soviet Academy of Sciences in a letter widely quoted by the American press. He referred to the actions of the leaders of the Soviet Academy as "disgraceful" and compared them to Nazis. Dobzhansky, meanwhile, had prepared the requested statement for the signature of the executive committees of the GSA and the ASHG. Muller had originally suggested that the signed statement be printed in *Science*, or the *New York Times*, or both.³⁴ Muller was so taken with the statement, however, that he attempted to use it to rally a larger fraction of the biological community. Rather than simply sign the statement himself and hope for Snyder's signature, Muller shared it with his Indiana University colleague Ralph Cleland, the chairman of the governing board of the American Institute of Biological Sciences.³⁵ As the professional umbrella group for the biological sciences, the AIBS nominally represented the views of 80,000 American university scientists, government researchers, and college and high school teachers. The endorsement of either the governing board of the AIBS or its constituent societies would demonstrate

33. The standard biography of Muller is Elof Alex Carlson, *Genes, Radiation, and Society: The Life and Work of H. J. Muller* (Ithaca: Cornell University Press, 1981). A concise discussion of his changing political views can be found in Diane B. Paul, "H. J. Muller, Communism, and the Cold War," *Genetics* 119 (1988): 223–25. Muller's involvement with liberal anti-Communist New York intellectuals is rarely commented upon but is evident in his correspondence and professional memberships. See also Hook, *Out of Step* (ref. 12).

34. Muller to Dobzhansky, 27 Sep 1948, HJM.

35. Muller to Dobzhansky, 3 Nov 1948, HJM.

with certainty the American biological community's opposition to Lysenko's scientific theories.

The original statement that Dobzhansky wrote for Muller dismissed Lysenko's theories as anachronistic and unscientific. Unlike Dobzhansky's translation of *Heredity and Its Variability*, which merely presented Lysenko's scientific experiments and left the reader to draw his or her own conclusions, the AIBS statement proclaimed Lysenko to be a fraud. His experiments had been conducted in a way which clearly demonstrated "that Lysenko is either unfamiliar with, or else is willfully ignoring, the basic facts and the methods of investigation of the sciences which he presumes to negate." Such behavior demanded that American scientists denounce Lysenko's work as pure charlatanism: "It is in reality a conflict of outworn notions closely approaching superstition against science."³⁶ While the statement stopped short of actually condemning Lysenko's political power, it castigated the sort of government that elected not only to decide scientific controversies but also dismiss dissenting scientists.

The AIBS's governing board made several critical revisions before circulating Dobzhansky's statement to the American Society of Naturalists, the Society for the Study of Evolution, the ASHG, and the GSA. Their revised statement, which directly referred to "official Communist views on heredity," encountered substantial resistance within the GSA's executive committee. The dissenting members of the committee felt the document overstated the case, but more importantly, they questioned the authority of a professional society to issue a political statement on behalf of its members. *Science*, moreover, declined to publish the statement until July of the following year, severely limiting its effectiveness. The AIBS's difficulty in gaining acceptance and publicity for such a seemingly obvious action—the condemnation of the hijacking of science by politics—deserves further explanation.

It had become clear within two weeks that the GSA's executive committee would not reach consensus. The 1948 executive committee included L. H. Snyder (president), Tracy Sonneborn (vice president), M. R. Irwin (secretary), George W. Beadle, Curt Stern, and Muller. Of these six, Beadle and Stern objected to the GSA's support of the statement.³⁷ Beadle and Stern's objections

36. Draft AIBS statement, [written by Dobzhansky] attached with annotations to Muller to Dobzhansky, 3 Nov 1948, HJM.

37. M. R. Irwin to Ralph Cleland, 22 Nov 1948, GSA, APS, Box 6, Folder AIBS #2, 1948. Beadle's objection letter is located in the APS collection: Beadle to Irwin, 17 Nov 1948, GSA, Box 6, Folder Beadle 1948. Although Stern's objection letter is unavailable, the sequence of letters strongly suggests that he must have cast the second protest vote. On November 18, 1948, before

underscored the obstacles to a scientific organization's involvement in political activities. While Beadle more or less agreed with the statement itself, he felt the entire society, rather than simply the executive committee, should approve the statement. Stern, on the other hand, objected to the statement's "politically colored sentences" and "unverified statements." Both thought the document had overstated the case.³⁸

Nevertheless, at some point in the spring of 1949 (the record is unclear as to exactly when or why), the GSA's executive committee relented and agreed to endorse the document. There was now a new problem: George Baitsell, the editor of *Science*, refused to publish it, on the grounds that the journal had already published too much on the topic.³⁹ *Science* did eventually publish the statement in June, nearly ten months after Lysenko's triumph, but for Muller, this experience only reinforced his conviction that geneticists needed to aggressively market their individual opinions.⁴⁰ Indeed, Muller saw a vast conspiracy in Baitsell's decision to delay publication and spread the blame to Bentley Glass, his former student and the current representative for biology on the AAAS editorial committee. Calling Glass's failure to stand up for the geneticists' interests "very reprehensible," Muller claimed that Glass "obviously let Baitsell put it over on him, believing Baitsell's pretexts and not seeing that Baitsell was really anti-genetic himself."⁴¹

Muller's interpretation notwithstanding, the GSA executive committee's reluctance to endorse the statement most likely grew from their fear of appearing dogmatic, not from secret commitments to Communism.⁴² And indeed,

he could have received Beadle's letter, Irwin assured Sonneborn that he had heard only positive responses but that he had not yet heard from Stern. Irwin to Sonneborn, 18 Nov 1948, GSA, Box 6, Folder Stern 1948. Four days later, Irwin reported the Executive Committee's votes to Cleland as four in favor, two against. Irwin to Cleland, 22 Nov 1948, GSA, Box 6, Folder AIBS #2. The other possibility—unlikely but not impossible given Irwin's later actions—is that Irwin disingenuously led Sonneborn to believe that he (Irwin) would approve the measure, then later opposed it.

38. M. R. Irwin to Ralph Cleland, 22 Nov 1948; and Beadle to Irwin, 17 Nov 1948; both in GSA, Box 6, Folder AIBS #2, 1948.

39. Muller to Edgar Altenburg, 16 May 1949; Muller to Robert C. Cook, 17 May 1949; both in HJM.

40. E. G. Butler, T. C. Byerly, F. P. Cullinan, W. O. Fenn, and R. E. Cleland, "A Statement of the Governing Board of the AIBS," *Science* 110 (1949): 124–25.

41. Muller to Sonneborn, 22 Jun 1949, HJM.

42. Sapp, *Beyond the Gene* (ref. 1), 168–80, discusses American geneticists' fears that any action or statement that could be interpreted as establishing a scientific dogma might be used by Lysenko's allies as evidence that Western science was based on ideological principles rather than the scientific method.

news coverage of the statement seemed to prove them correct. The editorial section of the *New York Times* the week following the statement's publication carried a brief story that quoted only its most inflammatory elements. According to science editor William Laurence's gloss, "The attack by Soviet scientists on Mendelian genetics 'does not represent a controversy of two opposing schools of scientific thought' but 'is in reality a conflict between politics and science.' . . . 'Genetic researchers,' the statement reads in part, 'definitely support the reality of the gene . . . and the validity of Mendel's laws.'"⁴³

Muller, meanwhile, continued his own one-man campaign. Muller's well-publicized resignation from the Soviet Academy and his recent Nobel Prize made editors receptive to his queries, and in December 1948, the *Saturday Review of Literature* published Muller's version of the Lysenko controversy in a two-part article. Each half was featured as the cover story. Provocatively titled "The Destruction of Science in the USSR" and "Back to Barbarism—Scientifically," Muller's essays offered vitriol rarely matched in more scholarly publications. The first article included a brief history of the controversy as Muller saw it. Lysenko's influence—referred to by Muller as "the wilful destruction of science in the USSR by politicians"—was a "brutal attack on human knowledge." Unlike the AIBS statement or Dunn's careful criticisms, Muller did not attempt to represent Lysenko's theories as "scientific." "To a scientist," Muller wrote, "Lysenko's writings along theoretical lines are the merest drivel. He obviously fails to comprehend either what a controlled experiment is or the established principles of genetics taught in any elementary course in the subject." Instead of pursuing science, Lysenko and his followers offered "a naïve and self-contradictory party dogma." Finally, Muller declared Lysenkoism "a superstition, as much a superstition as a belief that the earth is flat." Such superstitions must be eradicated, Muller claimed, if humanity were to advance.⁴⁴

The second article more ominously described the threat of Lysenko beyond the boundaries of the Soviet Union, including the United States—"particularly in the sections where Communist influence is strong." Foreshadowing his conflict with *Science* magazine, Muller claimed that scientists who had attempted to criticize Lysenko "have had considerable difficulty getting their articles published, even in the USA and Britain." The article explained the ideological underpinnings of dialectical materialism and the "perennial existence within

43. Editorial, *New York Times*, 7 Aug 1949.

44. H. J. Muller, "The Destruction of Science in the USSR," *SRL*, 4 Dec 1948, 13–15, 63–65, quotes on 13, 13, 13, 14, and 65.

the USSR of an emotional state resembling war hysteria,” both of which, according to Muller, required unyielding subservience to authority. But the danger was not limited to the Soviet Union. Illustrated with a photograph of Clarence Darrow and William Jennings Bryan, the well-known protagonists of the Scopes trial, the article warned that a scientific illiteracy enforced by religious fundamentalists had fostered a “popular misunderstanding” of genetics and heredity. Indeed, Muller warned, the failure of schools to adequately teach biological evolution had produced an ignorant and gullible population, a mistake that “may at some future time facilitate the rise of Lysenkoism and other dangerous anti-scientific movements.”⁴⁵

To Muller’s consternation, the *Saturday Review* received many letters criticizing both the content and the tone of his articles. S. F. Thomas, of Palo Alto, California, “condemned” Muller’s “lack of scientific approach” and his “emotional taint.” Similarly, A. I. Friedman, of Hackensack, New Jersey, accused Muller of the “same sort of political invective which he accuses the USSR.” William S. Maxwell of Philadelphia agreed, commenting that he “was not favorably impressed by the vehemence with which Mr. Muller denounced Soviet theories of genetics, without once offering any really conclusive evidence of why he thought they were unsound.” Of the seven letters the *Saturday Review* published in response to Muller’s articles, only one offered unqualified support.⁴⁶ Muller encountered similar responses from readers when he engaged in a printed debate with George Bernard Shaw several months later.⁴⁷ These criticisms pointed to the flip side of Dunn’s limited approach to criticizing Lysenko. When Dunn simply focused on Lysenko’s science, he worried that readers might miss the point; but when Muller focused exclusively on Lysenko’s politics, he drew criticism for leaving the sphere of science.

In a fascinating exchange of letters in early 1949 that echoed the battles of the American left in the 1930s, Muller and Dunn compared their views on how they should approach the Lysenko problem. Muller thought Dunn’s criticisms of Lysenko did not go far enough, and he urged Dunn to overcome his sympathy for the Soviet position. Writing as one old socialist to another, Muller chided Dunn, “Let me say that I do not think there is the slightest chance for reconciliation with the Soviet authorities over this matter—it has been tried

45. H. J. Muller, “Back to Barbarism—Scientifically,” *SRL*, 11 Dec 1948, 8–10, quotes on 8 and 9.

46. Letters to the Editor, *SRL*, 8 Jan 1949, 23–24.

47. H. J. Muller, “It Still Isn’t a Science,” *SRL*, 16 Apr 1949, 11–12, 61; and George Bernard Shaw, “Behind the Lysenko Controversy,” *SRL*, 16 Apr 1949, 10–11.

for some thirteen years now—and I think all which is left is to call a spade a spade.” Although he ceded Dunn’s point that Lysenkoism differed significantly from Nazi genetics—a claim Muller made on multiple occasions that particularly irritated Dunn—he worried that Lysenkoism presented a “slippery slope” for totalitarian regimes.⁴⁸ Dunn thanked Muller for his comments and concerns, but reiterated his belief that American geneticists were most effective when they limited their role to “pointing out the problem.” Instead of relentlessly criticizing either Lysenko or the Soviet political system, Dunn preferred that they “try to discover and describe just what those views were and are.”⁴⁹

Regardless of their differing philosophies on the most appropriate way to counter Lysenko’s influence, both Muller and Dunn were successful at finding outlets for their views. Speaking as individuals rather than scientists, they condemned Lysenko’s power in the Soviet Union. Dunn, who was not quite ready to give up on the Soviet experiment, particularly wanted to prevent the development of a stereotype of Soviet science; Muller, on the other hand, feared the arrival of Communist authority on American shores. They found it more difficult to integrate their views—and those of their colleagues—within the framework of a professional organization.

THE PROFESSIONAL POINT OF VIEW: COMMEMORATION AS POLITICAL WEAPON

While the GSA’s executive committee considered the fate of the AIBS statement, Tracy Sonneborn, the acting president, suggested that the organization create a committee to address the problem directly. Sonneborn had himself become involved in the controversy, mostly against his will. In late 1948 Sonneborn discovered that Communists abroad were using his research on cytoplasmic inheritance to bolster support for Lysenko’s theories. An avid anti-Marxist since college, Sonneborn was furious when he then learned that several daily and weekly newspapers, including *Newsweek* and the *New York Post*, had used his work to suggest some of Lysenko’s work might be valid. What was more, the British cytologist Cyril Darlington told Sonneborn that European papers portrayed his work as directly in support of Lysenko’s stance. These rumors compelled Sonneborn to respond, and in October, he recorded a broadcast for

48. Muller to Dunn, 17 Jan 1949, LCD, Folder Lysenko Controversy in the U.S. #4.

49. Dunn to Muller, 17 Feb 1949, LCD, Folder Lysenko Controversy in the U.S. #1.

distribution over the Voice of America network. Whereas Muller and Dunn condemned Lysenko's science outright, Sonneborn found himself in the awkward position of distinguishing the subtle differences between his and Lysenko's criticisms of Mendelian genetics.⁵⁰

Sonneborn decided to enlist help in his battle against the Communists—hence the creation of the wonderfully named “Committee to Counteract Anti-Genetics Propaganda” (CCAGP). Sonneborn envisioned his committee as a public relations clearinghouse, so he suggested appointing members who would be ready and able to speak to a broad audience. Besides Muller and Dobzhansky, Sonneborn indicated a preference for Robert C. Cook and Bentley Glass. Cook and Glass, he explained, “could help a great deal” through their editorial roles at *Science*, *Scientific Monthly*, and the *Journal of Heredity*.⁵¹ Cook edited the latter and Glass had recently joined the editorial committees of *Scientific Monthly* and *Science*. In suggesting that the committee recruit Cook and Glass specifically for their editorial roles, Sonneborn admitted that their plan to publicly condemn Lysenko might face resistance within the scientific community. It is, however, telling that no one at the GSA thought to enlist the services of either a professional press agency or Science Service, a news syndication service with institutional ties to AAAS and other science organizations.⁵²

By February 1949 the committee was up and running.⁵³ All four of Sonneborn's nominees accepted membership, and the group began to monitor the American and foreign press closely for signs of expansion in Lysenko's power. They compiled bibliographies, wrote op-ed pieces, and personally lobbied their colleagues to take stronger action. Muller's special contribution was to organize a six-part Voice of America series featuring himself and five other geneticists.⁵⁴

50. Sapp, *Beyond the Gene* (ref. 1), 168–80, discusses Sonneborn's defensiveness on the topic of Lysenko.

51. Sonneborn to M. R. Irwin, 15 Nov 1948, GSA, Folder Sonneborn #2, 1948.

52. For the origins and development of Science Service and its role as a public relations agency for the scientific community, see Marcel Chotkowski LaFollette, *Science on the Air: Popularizers and Personalities on Radio and Early Television* (Chicago: University of Chicago Press, 2008), particularly chap. 3, “Syndicating Science.”

53. Although I have been unable to locate either Sonneborn's invitation or initial correspondence in the GSA papers, Muller mentions the group's existence in a letter to Julian Huxley in February. Muller to Huxley, 10 Feb 1949, HJM.

54. Karl Compton, the chairman of the influential Research and Development Board, personally asked Muller to organize the session. That Compton, rather than a representative of the United States Information Agency, made contact is highly unusual and suggests the importance that science policymakers placed on the Lysenko situation. The letter itself arrived on Pentagon stationery and was marked “Restricted.” Each recipient received \$40 for his participation. See

Almost immediately, however, two issues clouded their mission. First was the case of geneticist Ralph Spitzer at Oregon State College. Spitzer, a young, untenured faculty member with Communist sympathies, had been accused of teaching Lysenko's theories to his undergraduate students. The Federation of American Scientists defended Spitzer on the basis of academic freedom in American classrooms, neglecting to comment on scientific freedom in the Soviet Union. The case forced the committee members, and especially Muller, to choose between the conflicting agendas of promoting academic freedom and condemning Lysenkoism.⁵⁵

The committee encountered a more structural problem in the definition of their responsibilities. Echoing their earlier concerns about the propriety of signing the AIBS statement without the full membership's approval, the executive committee prevented the committee from speaking on behalf of the GSA. The GSA's treasurer-secretary, M. Robert Irwin, who had been attempting to limit the committee's powers since its inception, made his particular opposition known at the annual meeting of the GSA in December 1948. The stalemate continued throughout 1949, even as Sonneborn convinced the executive committee that some sort of public platform had become increasingly necessary. His impassioned letter requested the creation of a new committee with "representatives authorized to speak and act in the name of the Society on matters of sufficient importance to warrant Society action or expression of opinion." Although he mentioned the Lysenko controversy, he also pointed to political investigations of geneticists and ongoing concerns about genetic radiation. His letter is worth quoting at length for its comparisons to broader forms of governance:

I am well aware that such a Committee, no matter how carefully selected, could never hope to have the complete agreement of the Society on its views and actions. Nevertheless, this seems to be insufficient reason to oppose the idea. In a

Compton to Muller, 1 Jul 1949; Muller to Compton, 6 Jul 1949; Muller to F. H. Richardson, 26 Jul 1949; Muller to Cook, 3 Nov 1949; and Muller to Robert C. Cook, Ralph Cleland, Bronson Price, Conway Zirkle, and Tracy Sonneborn, 9 Dec 1949; all in HJM.

55. Although both Sapp, *Beyond the Gene* (ref. 1), 177–79, and Carlson, *Genes, Radiation, and Society* (ref. 33), 330–32, discuss the Spitzer case as one of the major obstacles to the committee's success, I found very little mention of this issue in the GSA's papers. Muller mentioned his displeasure at an American Federation of Scientists editorial in a letter to Leo Szilard, and Glass alluded to it in the committee's first report, but its main importance seems to have been in distracting the committee members from overcoming opposition within the GSA. See Muller to Szilard, 21 Jul 1949, HJM; and Robert C. Cook, Theodosius Dobzhansky, H. J. Muller, and Bentley Glass, "Report of the Committee to Counteract Anti-Genetics Propaganda," 30 Nov 1949, GSA, Box 6, Folder Committee to Counteract Anti-Genetics Propaganda 1949.

democracy we do not expect 100% agreement on foreign or domestic policy, yet we have representatives who act and speak for the whole nation. If we were to insist on 100% approval before action were taken, either in Government or in our Society, there would be no action at all. The dissident members will, moreover, always have the right and opportunity to express publicly their dissent.⁵⁶

After complex negotiations within the society and a general election at the December 1949 meeting, it was eventually agreed that the CCAGP would be folded into a “Committee of Nine,” consisting of themselves and the executive committee, and that would in turn design a successor committee to broach the broader topic of scientific freedom and public education. The original four members might or might not form the core of the resulting committee, depending on the results of a general election of the GSA membership.⁵⁷ Moreover, the committee would only be allowed to speak for itself, not on behalf of the Society at large.⁵⁸ Muller and Cook agreed to continue to serve, albeit reluctantly. Muller “doubt[ed] very much whether, under the hampering conditions imposed at the last meeting of the Society, the Committee can any longer accomplish anything of value.”⁵⁹ Cook seconded with a loud “AMEN!” scrawled across his letter.⁶⁰ Glass, while less adamant, held similar doubts.⁶¹

The list of nominees circulated in the fall of 1950 included Cook, Glass, Muller, Stadler, Joshua Lederberg, Salvador Luria, G. Leonard Huskins, Alfred Sturtevant, and H. H. Plough. (Dunn’s name was notably absent.) Each nominee’s statement explained what roles the candidate envisioned for the committee. Though all of the candidates endorsed the idea of an anti-Lysenkoist campaign, they differed on the proper role for geneticists in a public controversy. On the

56. Sonneborn to the GSA Executive Committee, 1 Jun 1949, GSA, Box 6, Folder 1949—Executive Committee #1.

57. W. R. Singleton to Muller, Dobzhansky, Glass, and Cook, 5 Feb 1950, GSA, Box 7, Folder Committee to Counteract Anti-Genetics Propaganda. Singleton replaced Irwin as the GSA’s secretary-treasurer.

58. Irwin to Sonneborn, 21 Jan 1950; and Everett Dempster to Sonneborn, 17 Jan 1950 (on the amendment limiting the new committee’s powers); both in GSA, Box 6, Folder 1949—Sonneborn, T. M.

59. Muller to W. R. Singleton, 17 Feb 1950, GSA, Box 7, Folder Committee to Counteract Anti-Genetics Propaganda.

60. Robert C. Cook to W. R. Singleton, 24 Feb 1950, GSA, Box 7, Folder Committee to Counteract Anti-Genetics Propaganda.

61. Glass to W. R. Singleton, 16 Feb 1950, GSA, Box 7, Folder Committee to Counteract Anti-Genetics Propaganda. It is unclear whether Dobzhansky participated in the “Committee of Nine”; his response is unavailable and he was living in Brazil at the time.

most mundane level, Huskins, Stadler, and Sturtevant stressed the importance of clarifying the committee's relationship with the GSA as a whole. Muller and Cook, predictably, warned of dire consequences if the committee had to wait for GSA approval. Glass, Lederberg, and Plough, on the other hand, warned that the committee should be careful not to establish its own brand of dogma in the process of combating Lysenkoism. Luria, the odd man out, suggested the committee take a broad view of public education and mount a campaign against racism in American society.⁶² The GSA membership apparently agreed with Muller, and he, Sturtevant, Glass, Cook, and Stadler (in order of votes received) were elected to the committee in December 1950.⁶³

To put this another way: it took the GSA nearly two years—from the time that Sonneborn, as acting president, proposed a committee authorized to speak on public issues—to create it. During this time, however, the GSA was hardly silent on the issue of Lysenkoism. Instead of endorsing its members' anti-Lysenkoist activities, the society orchestrated an event more palatable to its controversy-shy members: a massive celebration of the achievements of Western genetics, timed to coincide with the fiftieth anniversary of the rediscovery of Gregor Mendel's laws.⁶⁴ While their *public statements* celebrated scientific cooperation and achievement, their *correspondence* constantly referred to the Lysenkoist threat.

There was now a third committee: the Golden Jubilee Committee, designed explicitly as a professional alternative to Muller and Cook's confrontational tactics. The GSA's new president, Curt Stern, enthusiastically endorsed the idea; Irwin, the former secretary-treasurer, would chair, joined by four other moderates (including Dunn). The committee's main responsibility was to celebrate Mendel's achievements. In practice, this meant the development of appropriate events and materials for the 1950 GSA meeting, including an educational pamphlet and a publicity campaign. Irwin offered the Golden Jubilee Committee to his peers as a moral model of how to deal with the media, the public, and their scientific colleagues. Muller's frantic, doom-filled letters only reinforced Irwin's belief that

62. "Views of Nominees on the Work and Function of the Public Education and Scientific Freedom Committee" [n.d. but mailed to GSA members 7 Nov 1950], GSA, Box 7, Folder AIBS #1.

63. "Results of the Voting for the Committee on Public Education and Scientific Freedom" [n.d., Dec 1950], GSA, Box 7, Folder Committee on Public Education and Scientific Freedom.

64. For insightful perspectives on commemorative practices, see Pnina G. Abir-Am and Clark A. Elliot, eds., *Commemorative Practices in Science: Historical Perspectives on the Politics of Collective Memory*, *Osiris* 2nd ser. 14 (1999).

unchecked elements within the GSA were dragging the organization into politics and scandal instead of advancing its scientific mission.

It was not that Irwin thought that the GSA should remove itself from the Lysenko controversy; rather, he disapproved of what he called the CCAGP's negative approach. As he phrased it in a letter requesting funding from the Rockefeller Foundation, "It is the definite feeling of this committee that the best answer to the anti-genetics propaganda is to make plans for a program at this Golden Jubilee which will put principal emphasis on the accomplishments of genetics in a very positive manner."⁶⁵ This particular wording was neither accidental nor specially tailored for a funding agency; both Irwin and Singleton used nearly identical language whether they were inviting members to join the committee, reporting to the GSA's officers, or corresponding with the local arrangements chair, corporate donors, a U.S. congressman, or the public relations firm hired to handle the event.⁶⁶ Neither man ever committed the *reasons* for his objection to paper; the merits of a positive rather than a negative campaign were apparently self-evident. Singleton's comments to Irwin in March of 1950 are typical: "Personally I should like to see the Committee under this name [the Committee to Counteract Anti-Genetics Propaganda] discontinued, and set up as a committee to give full publicity to the contributions that genetics is making. . . . I think we should not even grant the anti-genetics propaganda a place in the records of the Genetics Society of America, but that any committee which works to publicize the science of genetics should have a positive rather than a negative name."⁶⁷ The correspondence between Singleton and Irwin gives the distinct impression that they simply considered Muller's tactics distasteful: his positions were almost beside the point. In hoping to avoid controversy, their views were not so different than those of Dunn, who despaired of worsening relationships between the United States and the Soviet Union; but they also reflect an assumption that professional societies should not be in the business of furthering public controversy.⁶⁸

65. Irwin to Warren Weaver, 8 Mar 1950, GSA, Box 7, Folder 1950—Golden Jubilee, M. R. Irwin Correspondence #2.

66. See, for example, Irwin to Paul Mangelsdorf et al., 22 Feb 1950, Folder Golden Jubilee Correspondence #8; Irwin to Singleton, 24 Feb 1950, Folder Golden Jubilee Correspondence #1; Irwin to Stern, 21 Mar 1950, Folder Golden Jubilee Correspondence #8; Irwin to D. C. Rife, 6 Apr 1950, Folder Golden Jubilee Correspondence #10; W. R. Singleton to W. Kingsland Macy, 9 Mar 1950, Folder Golden Jubilee Correspondence #1; all in GSA, Box 7.

67. Singleton to Irwin, 2 Mar 1950, GSA, Box 7, Folder Golden Jubilee Correspondence #8.

68. Singleton's correspondence reveals an obsession with a positive group image. See, for example, W. R. Singleton to Bentley Glass, 21 Feb and 18 Jul 1950; Singleton to H. J. Muller, 28 Feb

The 1950 GSA meeting in Columbus, Ohio, formed the centerpiece of the Golden Jubilee Committee's plans. While the first day of the four-day meeting focused on research papers, the remaining three days celebrated the achievements of Mendel and genetics. Richard Goldschmidt, one of the father figures of genetics, kicked off the proceedings with a keynote address on "The Impact of Genetics on the Biological Sciences." Other key speeches addressed historical aspects of genetics, the physical basis and physiology of the gene, cytogenetics, medical genetics, and agricultural genetics. The presidents of both the American Society of Naturalists and the ASHG dedicated their addresses to the topic as well. Finally, Julian Huxley, the famed British biologist and past secretary-general of UNESCO, capped off the ceremonies by discussing "Genetics, Evolution, and General Thought."⁶⁹ Several of the papers presented at the meetings were later collected and published as an edited volume under Dunn's direction.⁷⁰

The committee's publicity plans ranged from the predictable, such as the edited volume, to the creative, such as Singleton's attempt to commemorate Mendel with a postage stamp.⁷¹ The GSA's most innovative move was to hire the Pendray and Leibert Company, an industrial public relations firm that had worked with

1950; all in GSA, Box 7, Folder Committee to Counteract Anti-Genetics Propaganda; Singleton to Congressman W. Kingsland Macy, 9 Mar 1950, GSA, Box 7, Folder Golden Jubilee of Genetics, #1; and Singleton to M. R. Irwin, 2 Mar 1950, GSA, Box 7, Folder Golden Jubilee of Genetics, #7. Unlike their colleagues, Irwin and Singleton refrained from discussing their political views in their professional correspondence. Irwin, a well-respected scientist who pioneered the field of immunogenetics, regarded his work on domestic animals as eminently practical, and seems to have had little patience for what he considered political grandstanding. For information on Irwin's scientific career, see Ray D. Owen, *Malcolm Robert Irwin, 1897–1897: A Biographical Memoir* (Washington, DC: National Academy of Science, 2007); available online at: <http://books.nap.edu/html/biomems/mirwin.pdf> (last accessed 2 Sep 2009).

69. "Genetics, the First 50 Years," press release on the upcoming GSA meeting [n.d., 1950], GSA, Box 7, Folder Golden Jubilee, #5.

70. L. C. Dunn, ed., *Genetics in the 20th Century: Essays on the Progress of Genetics During Its First 50 Years* (New York: Macmillan, 1951).

71. Singleton's letter to his congressman requesting the postage stamp echoed the stereotypical image of the Lysenko controversy favored by the news media but simultaneously reiterated his insistence on positive action: "From time to time there crops up some anti-genetics propaganda, mostly from foreign sources, and in the U.S.S.R. genetics as such has been practically abandoned. The Executive Committee of the Genetics Society feels that a strong program of publicizing the accomplishments of Genetics will do more to counteract any such propaganda than anything else we can do. I think this semi-centennial celebration is an excellent opportunity for bringing forth the accomplishments of the science of Genetics as a pure science and also in plant and animal improvement." Singleton to Congressman W. Kingsland Macy, 9 Mar 1950, GSA, Box 7, Folder Golden Jubilee, Correspondence #1.

Brookhaven National Laboratory and the Westinghouse Corporation, to oversee their operations.⁷² This extraordinary effort was made possible by a sleight of hand involving a \$7,500 grant from the Rockefeller Foundation, originally intended for a publication; although two publications were eventually released, both were mostly an excuse to hire Pendray and Leibert as the GSA's publicist.⁷³ Dunn handled all of the publicity arrangements, including negotiating the contract with Pendray and Leibert and securing the services of his friend John Pfeiffer, a former science editor for CBS, to write a popular "Penguin-style" pamphlet on the accomplishments of Western genetics.⁷⁴ With input from Dunn and Singleton, the publicity firm issued news releases, mailed pitch letters, created advertisements, and compiled press kits for the Golden Jubilee celebrations. By November 1950, over 22,100 copies of the pamphlet had been sold—numbers undoubtedly increased by Pendray and Leibert's efforts to negotiate bulk sales to schools, libraries, and agricultural businesses such as Pioneer Hi-Bred Corn Company and DeKalb Hybrid Seed Company.⁷⁵ According to the company's report to Dunn, their efforts successfully ensured that the "Genetics Golden Jubilee dominated the picture in Columbus, Ohio. . . . The Golden Jubilee activities of the Society were, in our judgment, successful in their purpose of focusing widespread attention on the

72. Although the GSA's members seem not to have been aware of this, G. Edward Pendray, one of the firm's principals, had coordinated an Ad Council campaign on behalf of the Federation of Atomic Scientists in 1946 and 1957. For an excellent analysis of this campaign, see Megan Barnhart, "Selling the International Control of Atomic Energy: The Scientists' Movement, the Advertising Council, and the Problem of the Public," in *The Atomic Bomb and American Society: New Perspectives*, ed. Rosemary B. Mariner and G. Kurt Piehler (Knoxville: University of Tennessee Press, 2009), 103–19.

73. Dunn to Robert McDevitt, 12 May 1950, sums up a meeting about Pendray and Leibert's services. Dunn explained, "It was our opinion that the fund at our disposal was given for publication and that we are not at liberty to expend it for public relations as such. We understood, however, that a certain amount of public relations work would be involved in the publication and marketing of such a book and we thought that you might be able to suggest ways of giving such service as we can afford to pay for under the general head of publications." Located in GSA, Box 7, Folder Golden Jubilee Publications #1. For background on the Rockefeller Foundation's support of American genetics, see Robert E. Kohler, *Partners in Science: Foundations and Natural Scientists, 1900–1945* (Chicago: University of Chicago Press, 1991).

74. Dunn to Irwin, 19 Apr 1950, Folder Golden Jubilee M. R. Irwin Correspondence #10; and Dunn to Irwin, 3 May 1950, Folder Golden Jubilee M. R. Irwin Correspondence #11; both in GSA, Box 7.

75. Nichols Poultry Farms, Inc., purchased 2,500 copies; Moew Seed Company, Pioneer, and Dryden Poultry Breeding Farm bought 1,000 each; and DeKalb and J. C. Robinsons Seed Company settled for 100 and 50 copies, respectively. "Summary of Orders to November 13, 1950," attachment to Robert McDevitt to L. C. Dunn, 16 Nov 1950, GSA, Box 7, Folder Golden Jubilee, Publications #1.

fundamental contributions of modern genetics, and the role of Mendel as founder of the field.” Unfortunately, the national newspaper clippings from the celebration have since been lost, but it is clear that the firm succeeded in drawing significant media attention to the Golden Jubilee program.⁷⁶

The *New York Times*, for one, featured daily stories on the Golden Jubilee. None of these directly mentioned either Lysenko or Soviet genetics; the articles instead focused on the practical applications of and recent experimental evidence for the gene. They did, however, make subtle attempts to counter charges of scientific dogma. In language surely adopted from a Pendray and Leibert press release, one article quoted Snyder to the effect that “one of the most far reaching” mistaken beliefs about heredity held that “if a certain trait is demonstrated to have a genetic basis it is held not subject to environmental modification . . . and conversely, if a personal trait is shown to have been influenced by the environment many believe genetics had nothing to do with it.”⁷⁷

Most of the firm’s press releases stressed positive achievements. Rather than focusing on so-called basic research, however, these releases not so subtly stressed the practical applications of Mendelian genetics. Without mentioning Lysenko’s name, most of the releases described improvements to American lives, such as increased agricultural productivity, the production of inexpensive antibiotics, and early diagnosis of some medical problems, reached through Western genetics. (Undoubtedly these sorts of accomplishments were of interest to the Rockefeller Foundation.) Another more pointedly explained that the pamphlet “is intended to furnish popular information on a science which is steadily contributing to increased health and prosperity in America.”⁷⁸ Not all of the press releases muzzled political commentary on Lysenko’s theories; one particularly heavy-handed document explained that the pamphlet provided all the background information needed to understand the “tempest in the scientific world.” The statement proclaimed that “The Russians deny the existence of genes, the biological architects of heredity,” but that “Mendelian geneticists . . . have been unable to get the results claimed by Lysenko. Unless the results can be duplicated elsewhere, the scientific verdict can only be ‘invalid.’”⁷⁹

76. “Public Relations Report to the Genetics Society of America on Golden Jubilee Activities and the Public Affairs Pamphlet,” second attachment to *ibid.*

77. *New York Times*, 13 Sep 1950. For additional news coverage, see *New York Times*, 12 Sep 1950, and 15 Sep 1950.

78. “Publicity Material for Golden Jubilee of Genetics Meetings,” packet containing all of Pendray and Company’s press releases [n.d., Nov 1950], GSA, Box 7, Folder Golden Jubilee.

79. *Ibid.*

The geneticists were generally pleased with Pendray and Leibert's efforts. Dunn praised the efforts of Robert McDevitt, the publicist who represented the GSA, in a letter to one of the company's principals, writing, "considerable success was attained in getting *the right kind* of notice of the Genetics Society anniversary meetings." Even though occasional newspapers, such as the *New York Times*, insisted on reporting on controversy instead of consensus, "we were particularly glad to avoid overemphasis upon the differences with the Russians and I think this was achieved."⁸⁰ Using a public relations firm allowed media-skittish scientists like Dunn to speak their minds without actually appearing in a byline or in an interview. Indeed, Dunn so enjoyed the experience of working with the firm that he expressed the hope that more scientific societies would follow their example.

If the enduring memory of Gregor Mendel as the founding father of genetics is any indication, the GSA's Golden Jubilee was wildly successful. Dunn, Irwin, and Singleton brought their positive message directly to tens of thousands of pamphlet readers; many more read the glowing newspaper reports of the proceedings. Because their statements rarely strayed from the achievements of the past and the promise of the future, their activities elicited few protests. Unlike Muller, who received caustic letters almost every time he published an article on Lysenkoism, the Golden Jubilee Committee received only praise. It does give one pause, however, to reflect on the GSA's rather circumscribed notion of success. Political action on behalf of scientific societies could only extend so far in Cold War America.

CONCLUSION

During the years immediately following the end of World War II, a brief flowering of scientific activism unfolded in the public sphere, as numerous studies of the so-called atomic scientists have clearly demonstrated.⁸¹ The Federation

80. Dunn to G. Edward Pendray, 21 Nov 1950, GSA, Box 7, Folder Golden Jubilee, Publications #2. Although scientific organizations occasionally hired public relations firms to publicize specific events or expeditions, this appears to be one of the earliest attempts of a scientific organization to use a PR firm to influence public opinion. For an exception, see Barnhart, "Selling the International Control" (ref. 71). LaFollette, *Science on the Air* (ref. 51), mentions a few earlier incidents in passing (for example, see p. 60).

81. Alice Kimball Smith, *A Peril and a Hope: The Scientists' Movement in America, 1945-1947* (Chicago: University of Chicago Press, 1965); Boyer, *Bomb's Early Light* (ref. 3); and Wang, "Problem of the Public" (ref. 2).

of American Scientists and the National Committee on Atomic Information attempted to reach the public directly through speaker's bureaus, information packets, radio appearances, and filmstrips, and they encouraged the public to participate in letter-writing campaigns in support of civilian control of atomic energy. Jessica Wang and Megan Barnhart have convincingly argued that these efforts should be seen in the context of a Deweyian conception of an associationist public, in which an active and informed citizenry could make rational decisions for the public good.⁸² In the case of the atomic scientists, public engagement proved short-lived, as the growing anti-Communist movement managed to squelch most debate by the end of the 1940s.⁸³ Simply put, physicists needed security clearances, and maintaining them required a clean political record. Those scientists who continued to influence public policy on the control and development of nuclear arms and energy largely did so as government advisors operating within a technocratic system, rather than as public intellectuals engaging in the public sphere.⁸⁴

But things could have been different for the sizable group of biologists who worked on projects less obviously useful for military applications. Although a fairly high proportion of biologists had—and continued to nurse—leftist affiliations, the kind of research they engaged in was less likely to require security clearances. Moreover, private foundations continued to sponsor genetics research well into the postwar period, reducing its dependency on federal grants. At the same time, geneticists found themselves largely on the margins of federal policy-making. Here was an issue—Lysenkoism—that would seem to be a matter of great political concern, but attracted little government attention aside from a few letters from a former Vice President known for his leftist sympathies and State Department interest in a short series of Voice of America radio broadcasts.⁸⁵ Moreover, given geneticists' ongoing commitment to Soviet-American

82. Wang, "Problem of the Public" (ref. 2); Barnhart, "Selling the International Control" (ref. 71).

83. Wang, *American Science* (ref. 2).

84. The literature on postwar scientists as political advisors is rich and voluminous. For good starting points, see Kevles, *The Physicists* (ref. 3); Herken, *Cardinal Choices* (ref. 3); and Ronald E. Doel, "Scientists as Policymakers, Advisors, and Intelligence Agents: Linking Contemporary Diplomatic History with the History of Contemporary Science," in *The Historiography of Contemporary Science and Technology*, ed. Thomas Söderqvist (Amsterdam: Harwood Academic Press, 1997), 215–44.

85. In addition to broadcasting Sonneborn's lectures, the Voice of America also apparently recorded the entire Golden Jubilee Celebration. See the thank-you note from Dunn to Roger Lyons, 29 Dec 1950, GSA, Box 7, Folder Golden Jubilee L. C. Dunn Correspondence #3.

friendship and the increasingly frosty relationship between the two countries, it is not clear that increased federal involvement would have aided the geneticists' goal of supporting their Soviet colleagues. Instead, they turned to the public. As Nikolai Krementsov has argued, part of their motivation was to provide ammunition for the geneticists still working within the Soviet Union.⁸⁶ From their correspondence, however, it is clear that American geneticists held a deep-seated belief in the power of an educated, democratically engaged public to influence science policy across the globe. The fact that they failed to offer any sort of explanation for *how* that public might wield its power does not make their insistence on the point any less historically relevant.

The Lysenko controversy was the first extended contact many geneticists had with high-profile media organizations. They were surprised when they encountered resistance to their nuanced message from publishers, readers, and their colleagues, and they made what they later considered to be mistakes. The lessons they learned—both as individuals and as professional renegades—influenced their interactions with the public long after Lysenko's fall from power.⁸⁷ Muller's encounters with the letter-writing public moreover revealed that scientific authority in the public sphere could be tenuous. While the public welcomed leading scientists' commentary on scientific issues, they demanded *scientific* justification for these positions. It was not enough, Muller learned, to use one's position as a scientist to condemn political events elsewhere. While both Muller and Dunn experienced significant criticism of their social and political views in the *Saturday Review of Literature*, no one criticized their attempts to explain American or Soviet science. Instead, readers questioned their political motivation or expertise. Taking a position on a controversial issue might be acceptable, but the educated public expected scientists to justify these views with seemingly objective evidence. The scientists' professional colleagues reinforced a limited role for scientists as political figures by actively discouraging political commentary. It might be acceptable for a scientist to court media attention, but only as an individual citizen rather than as an emissary of the scientific community.

86. Krementsov, "Second Front" (ref. 1).

87. Not coincidentally, many of the same geneticists involved with the Lysenkoism debate were involved with other public issues, including the space race, the biological effects of radiation, and educational reform. See Audra J. Wolfe, "Germs in Space: Joshua Lederberg, Exobiology, and the Public Imagination, 1958–1964," *Isis* 93 (2002): 183–205; and Audra Wolfe, "Speaking for Nature and Nation: Biologists as Public Intellectuals in Cold War Culture" (PhD dissertation, University of Pennsylvania, 2002).

Equally important, the geneticists learned that successful media campaigns required a sophisticated strategy. On their own, Dunn and Dobzhansky found it difficult to draw attention to their translation of Lysenko's work; using the help of a public relations firm, in contrast, the GSA secured coverage for their Golden Jubilee in almost every major American newspaper. Their experience with Pendray and Leibert suggested that media contacts, press kits, and professionally written news releases might be worthwhile investments. Although geneticists' opinions might hold sway on the printed page, the GSA's more savvy members recognized that they needed help in gaining access to those pages. And whereas op-ed pieces and Muller's diatribes alienated readers, seemingly innocuous public affairs pamphlets could transform public attitudes.

The grand irony at the heart of the American response to Lysenkoism is that the geneticists involved hoped to condemn a politically motivated scientific program without drawing too much attention to either their scientific or political objections. Moreover, both the individual scientists and the GSA hoped to wage this campaign by reaching as many people as possible in the popular press. What at first seems like a remarkably bull-headed or even perverse approach to conflict only makes sense when considered through a complex set of motivations that extend well beyond the self-interest typically associated with postwar science publicity: the geneticists described in this article were driven by personal friendships, political commitments, professional obligations, and an enduring belief in the power of individuals to shape public opinion. When geneticists voiced their opinions on radio programs, feature articles, and letters to the editor, they were not only speaking *to* the public, but speaking *as* members of the public. If we, as historians, want to know why scientists did these things, we must be willing to consider scientists as historical actors with multiple identities well beyond their professional affiliations.⁸⁸ In the case of Lysenkoism, that means recognizing that political and personal divisions shaped geneticists' individual and collective responses almost as much as, if not more than, the desire to establish scientific legitimacy and the institutional rewards that would accompany it. In arguing that we take seriously the question of personal motivation, I am not making an impassioned plea for a return to biography as the primary method for understanding scientists' actions. Rather, this account is offered in hope that

88. Moore, *Disrupting Science* (ref. 2) offers a much-needed brief for a broader view of scientists' motivations beyond mere resource allocation.

continued attention to the questions of “Why here?” and “Why now?” will help us to produce more satisfying accounts of scientists’ attempts to engage the public, both in the postwar period and in the broader history of science.

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