tradition of radicalism in the United States after 1865. "If President Conant would find 'American radicals,' . . . ." Mr. Destler maintains, "let him . . . . invite Henry A. Wallace to lecture at Harvard University on 'monopoly and free enterprise,'" for Wallace's New Dealism is in a direct line from a late nineteenth-century radicalism that was more systematic in thought and national in scope than it is commonly supposed to have been. The inadequacy of the present view, Mr. Destler believes, comes from the fact that historians have written of Populism largely from local records and too much under the influence of Turner's frontier thesis. A double distortion has resulted. They have seen in Populism a crazy quilt of reactions to specific social conditions rather than a school of thought. They have failed to see the back-and-forth of influence between rural and urban dissidence which made of Populism a good deal more than an isolated farmers' movement.

Mr. Destler is strikingly successful in reconstructing Populism as an integrated philosophy, as an "economic collectivism . . . . to solve the problem of monopoly, ... a faith and a creed as well as a program." He runs into more difficulties in his presentation of Populism as a "cross-fertilization" of national dissidence. Certainly there was a continuous interaction of ideas between urban and rural dissidents, and an intense effort on the part of Populist leaders to build an ideological bridge between them. Mr. Destler has established these points so firmly that no future student of the period can ignore them. Yet the upshot of his research is that the bridge did collapse. As he points out, "the greatest problem of ideological conflict" sprang from "the clash of indigenous Populism, produced by decades of cross-fertilization between urban and agrarian radical movements, with an imported, proletarian Socialism which made its first great appeal to English-speaking wage earners in America in the depression-ridden nineties." And, he might have added, the urban-rural clash was complicated by regional tensions that produced Algie Simonses who did not like Eastern radicals, Eastern radicals who saw only coonskin when they looked west, and Southern Tom Watsons who were ready to agree with both East and West.

The bridge across which Theodore Roosevelt, Wilson, and Franklin Roosevelt walked to power was not so much a national program as it was a combine of heterogeneous and sometimes contradictory elements from many dissident traditions. The more national parts of their programs were also the vaguest. Perhaps that is just why Mr. Conant called for a new national radicalism and why Mr. Destler is seeking a national radicalism in the past. If so, the search requires not only emphasis on a Henry Wallace's ties to tradition but also historical explanation of how the tradition produced the confusions of a Henry Wallace.

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A History of Banking Theory in Great Britain and the United States. By Lloyd W. Mints. Chicago: The University of Chicago Press, 1945. Pp. 319. $3.50.

The main shortcoming of the traditional histories of thought consists in their presenting ideas as if they existed in some sort of vacuum. In fact, ideas are living entities which must be understood as the result of continuous interaction

between reality and thinking. Or in other words, ideas come into being under the impact of a certain institutional setup, they develop because the setup changes; and on the other hand, the development of ideas contributes to the change of the external world. It seems to me that no history of banking theory can be written without studying at the same time the history of banking, and that no history of banking theory can be satisfactory which does not show that process of interaction between thought and reality.

Such, however, is not the goal of Mr. Mints. He is by training not a historian, but a theorist. He believes banking theory was dominated by what he calls the "real-bill doctrine"; he considers this situation a "scandal" (p. 10), and he sets out to remedy an evil. In so doing he undertakes a task which is essentially not that of a historian and from the outset impairs the adequacy of a book to which he has chosen to give the title *A History* . . . .

Approaching his subject as a theorist he presents theorems and subsequently criticizes them on the basis of 1945 knowledge (for example, pp. 78, 79, 98, 113, 134). Such a method seems to me both historically unjust and logically untenable. If our works should still be of interest to scholars in the year 2045, we would be entitled to be understood against the background of our time instead of being judged or condemned with the knowledge of that remote year; and the writers of the years 1745 and 1845 have certainly the same right today. Theories themselves are historical categories and a certain now-outdated opinion may once have been the correct expression of a reality that is no longer ours. I do not object to Mr. Mints's judging the theories he presents, for an idea is either correct or incorrect. But for the historian it is so in relation to the reality of a certain period and it must be judged understandingly with reference thereto. Only by such an outlook can the student see one of the most important aspects of any history of ideas, namely, the processes in which true ideas become untrue, in which false ideas are corrected, and in which valuable truths are lost.

Guided by his 1945 yardstick, Mr. Mints enumerates what everybody who has written a book or pamphlet on the theory of banking thought about various questions and to what extent he was "correct." No lines of influence are pointed out. There is no mention of the way in which American banking theory reflected the British while lagging behind it. The development of important ideas other than the real-bill doctrine is traced systematically through the decades only in exceptional cases, as for instance on pages 104 and 105 which treat the theories on "the manner in which banks of a competing group operate to expand the circulating medium." No distinction is made to show whether an author was influential or not, whether his opinion was widely adopted, perhaps the banking thought of the day, or whether it was the occasional flash of a monetary crank. But Mathew Carey, John McVickar, and Condy Raguet, for instance, were more important than Francis Bowen, James Cox, and Henry Vethake, and they deserve a different emphasis. Two men who in my opinion were among the most important American banking theorists of the first half of the nineteenth century are not discussed. One is Nicholas Biddle, the central banker, who was as much of a banking theorist as any central banker in the nineteenth and twentieth centuries. Although he did not write books and pamphlets on the subject, the material that shows him in his capacity as a theorist is extant in his letter books

(in the Library of Congress), in Congressional documents, and in his important article, "The Currency," in the *National Gazette* of April 10, 1828, which was reprinted repeatedly in contemporaneous publications. The other man to be slighted is Isaac Bronson, the very influential banker and banking theorist, who is confounded with his relative, the jurist Isaac H. Bronson.

In Mr. Mints's campaign against the real-bill doctrine, he describes it as follows: "If only 'real' bills (short-term business-paper) are discounted the expansion of bank money will be in proportion to any extension in trade that may take place, or to the 'needs of trade,' . . . . if only commercial loans are made the currency will have a desirable elasticity and the banks will . . . . be in a liquid condition" (p. 9). This theory has two aspects: First, it tells the banker that he should discount real paper only, if he wants to be liquid and to have an elastic circulation. (Mr. Mints reprints on p. 93 a quotation from Tooke's *History of Prices* which can be characterized as a classical expression of that point of view.) Secondly, the doctrine advises legislators and central bankers regarding the economic consequences of an exclusive discounting of real bills by bankers. I agree with Mr. Mints that the *economic* aspect of the real-bill theory is untenable, but I think that its adoption by the business of banking had the most wholesome consequences. In fact, the "elegant" elaboration of the real-bill theory by Adam Smith and its subsequent adoption (although with a time lag) by the practical banker mark the dividing line between mercantilist and modern banking practice. It is strange that Mr. Mints has overlooked the most important American eighteenth-century representative of the real-bill doctrine, James Sullivan who published anonymously *The Path to Riches*. The most influential American representative of the real-bill theory in the early nineteenth century was Isaac Bronson.

From one point of view, the weaknesses of Mr. Mints's book are those of superspecialization and therefore should be a warning to all of us. We should never forget that, while twentieth-century knowledge is departmentalized, reality to which it refers is not. If specialization leads to a point where we do not see our findings on their proper background and in their correct perspective, and if we do not present them with the proper emphasis, our work is in vain. Superspecialization in research, even if the details are 100 per cent correct, easily leads not to an understanding and a picture, but to a misunderstanding and a distortion of reality. If that happens, we may one day cease to be a blessing to society. But service to society is the only justification for our sitting in libraries instead of tilling the soil. If we forget that basic fact we and all our knowledge may be swept into limbo.

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William Morris: Medievalist and Revolutionary. By Margaret R. Grennan. New York: King's Crown Press, 1945. Pp. x, 173. $2.50.

There is a queer unreality about William Morris that is the harder to explain in view of the terrific energy and genuine idealism of that flamboyant mid-Victorian. It is very difficult now to get close to him. He accomplished a lot;