undermine simple binary conceptions of sex differences and chromosomal sex determination. There is no talk of mental blocks on Goldschmidt's part, but antifeminism, anti-Semitism, and racism are invoked to explain why other scientists rejected Goldschmidt and his conceptions in favor of the stable genes and hierarchies of mainstream genetics.

The reactionary Butenandt, blinkered by a binary view of the sexes, misguidedly set out to discover separate male- and female-determining hormones and always downplayed the fact that every individual secreted hormonal mixtures. He also placed hormones in gendered, hierarchical relationships with the cells and organs they supposedly controlled. That gendered hierarchy was transferred to the genes and their effects, because Butenandt classed genes with hormones as biologically active substances (*Wirkstoffe*). This precluded questions about gene–cytoplasm interactions from even arising.

The book ends with a call to get rid of hierarchies and degender our conceptions of the genes and all the cell components that generate biological differences. In Satzinger's story, the gender order has continually thwarted progress toward this goal, but it is a goal nonetheless, and it gives the book an underlying whiggishness. There is an implied directionality to the history of genetics, and it is always clear whether Satzinger thinks someone is advancing the program or being blocked or misdirected. Repeated juxtapositions of the wrong turns in genetics with turns toward Nazi racial ideology strike me as tendentious, even though no clear causal connection is ever articulated.

Despite the questionable interpretations, *Differenz und Vererbung* is impressive for the amount of research that went into it, especially into the reconstruction of so many scientific careers—not only of the leading scientists, but also of large numbers of assistants and collaborators, male as well as female, who were scattered by World War II. The book is meticulously documented, and it will be a great resource for historians of German biology. (For that purpose, a subject index would have been better than just a name index.)

SANDER GLIBOFF

Henning Schmidgen. *Die Helmholtz-Kurven: Auf der Spur der verlorenen Zeit*. 270 pp., illus., figs., bibl. Berlin: Merve Verlag, 2010.

Historians of science have returned frequently to Hermann Helmholtz's early career, fascinated by his remarkable contributions in physiology, physics, and psychology, in a context close to the Prussian political revolution of 1848 and the "takeoff" of German industrialization in the 1850s. Through a combination of surprising archival discovery and perspicacious interpretation, Henning Schmidgen has resolved some of the critical issues in a central theme of Helmholtz's early work: his use of graphical recording methods and time-measurement apparatus in his early physiological research.

Schmidgen introduces *Die Helmholtz-Kurven: Auf der Spur der verlorenen Zeit* with a thrilling account of the archival sleuthing that uncovered the critical documents in his story. In September 1851, Helmholtz's research on the velocity of nerve transmission was communicated to the French Academy of Sciences by his colleague Emil Du Bois-Reymond. Helmholtz's paper described his method of measuring the velocity of nerve transmission with self-registered curves, an alternative to his already well-known measurements "of the same facts" using a modified Pouillet ballistic galvanometer. Those facts—that the nerve impulse traveled at a speed of about 27 meters per second—had met with skepticism from a scientific community that had long assumed nerve transmission to be nearly instantaneous. With his "frog-drawing machine" Helmholtz recorded curves drawn by the frog's contracting muscle after its nerve had been electrically stimulated. The curves unfolded in three distinct phases, including a crucial latent phase of "lost time" (*temps perdu*) of about one-hundredth of a second. Helmholtz's experiment thus resolved a critical problem of long standing in physiology, psychology, and philosophy. But it also did much more. It offered a crucial demonstration of a new dynamic vision of life, and it foregrounded the new experimental technique that realized that vision in burgeoning new laboratories.

Schmidgen noticed that the curves themselves were omitted from the version of the paper published in the academy's *Comptes Rendus*, despite the fact that they were reported to have been "placed before the eyes of the academy." Wondering whether the curves had made it to Paris, and what had happened to them, Schmidgen traveled to Paris on a hunch that they might have been deposited in the archives of the academy. The "Helmholtz curves" turned out to be just where Schmidgen suspected. This exciting discovery of the curves in their full materiality provides the springboard for a possibly definitive account of Helmholtz's early use of graphical methods and techniques of physiological time measurement.

Schmidgen shows how Helmholtz's instrumental innovations grew out of a family of related apparatus—galvanometers, self-registering chronographs and dynamometers used in ballistics, telegraphy, and practical physics—that had been the subject of an international priority dispute in the 1840s. The original priority dispute gave rise to a second dispute among physiologists, pitting Helmholtz and Du Bois-Reymond against the Italian Carlo Matteucci on questions of animal electricity. The struggle pushed the physiologists deeper into the problems of instrumentation, with Helmholtz oscillating between experiments with graphical recording instruments and an electromagnetic method using a galvanometer. Helmholtz distrusted graphical methods as a means for measuring the speed of nerve transmission because of concerns that friction would distort the results. With advice from Carl-Friedrich Gauss and Wilhelm Weber, he devised a nearly friction-free galvanometric method, which deflected muscle contractions into the deflections of a needle through electromagnetic force. The deflections, read through a telescope placed a few meters away, yielded measures of remarkable precision. But the results failed to convince the key audience of the Paris academy, leading Helmholtz to return anew to the method of curves as a means of convincing skeptics. It was this improved experimental setup that proved triumphant in 1851. Schmidgen's descriptions of all of these experimental systems are exemplary; his account of the material techniques Helmholtz used to produce the permanent tracings is luminous.

But Schmidgen's masterful treatment of the micro-context may have come at the expense of the meso-context. Perhaps because of his focus on Helmholtz's research after arriving in Koenigsberg in 1849, Schmidgen ignores Helmholtz's interest, during his Berlin years, in certain instruments involved in the first priority dispute—specifically, the self-registering dynamometers of Morin and Poncelet that provided a model for his first attempts to obtain a graphical measure of the mechanical work expended in muscle contraction. In Schmidgen's account of Helmholtz's graphical methods, the Berlin moment appears to have been a kind of historical latency period. It might also mark the "lost time" that links Helmholtz's physiological research with his famous 1847 physics treatise *On the Conservation of Force*, as well as the political and economic contexts of 1848, two longstanding desiderata of historical explanation.

Schmidgen's Proustian subtitle, "On the Track of Lost Time," invokes the macrocontextual meaning of time and its measurement in the nineteenth-century scientific and cultural imagination. Although Helmholtz and Proust never met, Schmidgen suggests that the prominent appearance of the notion of *temps perdu* in their work was no uncanny coincidence. The scientist and the novelist were joined by pathways that made Helmholtz's methods into staples of experimental psychology's investigations of new perceptions, sensations, and thoughts, before Proust amplified them into a literary technique that made lost time knowable through the creation and decipherment of signs or traces. Schmidgen interweaves bits of the background to Proust's novel into his narrative, reminding us how much of nineteenth-century art was underwritten by science, in particular by recording instruments and related media. Schmidgen's sophisticated use of Gaston Bachelard, Gilles Deleuze, and other thinkers makes this often technical history of science a surprisingly good fit for a publisher best known for works by leading European philosophers and cultural theorists.

ROBERT M. BRAIN

Joel S. Schwartz. *Darwin's Disciple: George John Romanes, a Life in Letters.* xxi + 806 pp., illus., app., bibl., index. Philadelphia: American Philosophical Society, 2010. $60 (paper).

George John Romanes (1848–1894) was Charles Darwin's research associate for the last eight years of his life. Apart from the Darwin family, Romanes probably had more opportunity than anyone else to discuss evolution with the master. Their extensive written correspondence is central to this volume. Many of the letters are in Ethel Romanes's 1896 biography of her husband, which ran to several editions, and in the 1932 biography of John T. Gulick by his biochemist son, Addison.

The thinking of Romanes and Gulick, an American missionary in Hawaii, on the importance for branching evolution of some form of isolation between groups within a species converged in the 1880s. They supported each other against the attacks of the Darwinians, especially of Alfred Russel Wallace. The cogency of their arguments was for long unrecognized.

In the first edition of her biography, Ethel Romanes regretted that she had been unable to access the Gulick correspondence. Schwartz notes that this later became available to her but still was not included in subsequent editions. Yet some other late-arriving materials (e.g., correspondence with T. H. Huxley) were included, and other things that were present initially (e.g., correspondence with the director of Kew Gar-