

## Forum: Science in Translation

# Why Semmelweis's doctrine was rejected: evidence from the first publication of his results by Friedrich Wiegler, and an editorial commenting on the results

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**Abstract.** We present English translations of two French documents to show that the main reason for the rejection of Semmelweis's theory of the cause of childbed (puerperal) fever was because his proof relied on the *post hoc ergo propter hoc* fallacy, and not because Joseph Skoda referred only to cadaveric particles as the cause in his lecture to the Academy of Science on Semmelweis's discovery. Friedrich Wiegler (1821–1890), an obstetrician from Strasbourg, published an accurate account of Semmelweis's theory six months before Skoda's lecture, and reported a case in which the causative agent originated from a source other than cadavers. Wiegler also presented data showing that chlorine hand disinfection reduced the annual maternal mortality rate from childbed fever (MMR) from more than 7 per cent for the years 1840–1846 to 1.27 per cent in 1848, the first full year in which chlorine hand disinfection was practised. But an editorial in the *Gazette médicale de Paris* rejected the data as proof of the effectiveness of chlorine hand disinfection, stating that the fact that the MMR fell after chlorine hand disinfection was implemented did not mean that this innovation had caused the MMR to fall. This previously unrecognized objection to Semmelweis's proof was also the reason why Semmelweis's chief rejected Semmelweis's evidence.

### Source materials

The translation/s on which this article is based can be found under the 'supplementary materials' tab at <https://doi.org/10.1017/S0007087420000229>.

Anyone who speaks only English and tries to discover why Semmelweis's theory of the cause of childbed (puerperal) fever (MMR) was rejected is faced with the insurmountable problem that most primary documents probative of this question are written in German, Hungarian, French and Danish, and only a few have been translated into English. Consequently, although the Anglo-American literature on Semmelweis is extensive,

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almost all of it is based on secondary sources, and, perforce, has uncritically accepted answers to very important questions about Semmelweis and his work, not the least of which is why his theory received such a hostile reception in Vienna.<sup>1</sup> Only primary sources can reliably address this issue. Thus we have translated two French documents into English that are highly probative of this important question.

Semmelweis contended that every case of childbed fever was caused by ‘decaying animal-organic matter’ that in most cases was introduced into the birth canals of mothers in labour by the hands of their attendants, and that this could be prevented if the attendants disinfected their hands with a chlorine solution.<sup>2</sup> The most widely believed reason why Semmelweis’s theory was rejected by his contemporaries is that when Joseph Skoda gave a lecture to the Imperial Academy of Sciences about Semmelweis’s discovery (18 October 1849), Skoda mentioned only cadavers as the source of the causative agent. Any misunderstandings that ensued were then perpetuated by Semmelweis’s delay in publishing his results for eleven years.<sup>3</sup> Alternatively, and more recently, Tulodziecki, relying on earlier work by Loudon, suggested that Semmelweis’s doctrine was rejected because ‘he had already, unsuccessfully, insisted twice before that he had identified the only cause of puerperal fever’.<sup>4</sup> However, Wieger’s article, published on 20 April 1849 in the *Gazette médicale de Strasbourg*, six months before Skoda’s lecture, and the subsequent editorial comments on Wieger’s article, published in the *Gazette médicale de Paris* in 1850, flatly contradict both these claims.<sup>5</sup>

Friedrich Wieger (1821–1890) was an obstetrician from Strasbourg who visited Semmelweis at the end of May 1847, at the time when Semmelweis, then the assistant (equivalent of senior registrar or chief resident) introduced chlorine hand disinfection in

1 Irvine Loudon, ‘Semmelweis’, in Loudon, *The Tragedy of Childbed Fever*, Oxford: Oxford University Press, 2000, p. 100; Sherwin B. Nuland, *The Doctors’ Plague: Germs, Childbed Fever, and the Strange Story of Ignaz Semmelweis*, New York: W.W. Norton & Company, 2003; Theodore G. Obenchain, *Genius Belabored: Childbed Fever and the Tragic Life of Ignaz Semmelweis*, Tuscaloosa: The University of Alabama Press, 2016.

2 Ignaz Philipp Semmelweis, *The Aetiology, Concept, and Prophylaxis of Childbed Fever* (tr. Frank P. Murphy), Birmingham: The Classics of Medicine Library, 1981, pp. 439–441.

3 Joseph Skoda, ‘Ueber die von Dr. Semmelweis entdeckte wahre Ursache der in der Wiener Gebäranstalt ungewöhnlich häufig vorkommenden Erkrankungen der Wöchnerinnen und des Mittels zur Verminderung dieser Erkrankungen bis auf die gewöhnliche Zahl’, *Sitzungsberichte der mathematisch-naturwissenschaftlichen Classe der Kaiserlichen Akademie der Wissenschaften*, 1849, pp. 168–182, at [www.zobodat.at/pdf/SBAWW\\_03\\_0139-0186.pdf](http://www.zobodat.at/pdf/SBAWW_03_0139-0186.pdf); Ference A. Gyorgyey, ‘Puerperal fever 1847–1861: from the first statement about the discovery to the publication of Semmelweis’s aetiology’, MA thesis, Yale University, 1968; Loudon, op. cit. (1), p. 100; Nuland, op. cit. (1), pp. 123–124; K. Codell Carter and Barbara R. Carter, *Childbed Fever: A Scientific Biography of Ignaz Semmelweis*, New Brunswick, NJ: Transaction Publishers, 2005, p. 56; Obenchain, op. cit. (1), pp. 103–104.

4 Dana Tulodziecki, ‘Shattering the myth of Semmelweis’, *Philosophy of Science* (2013) 80, pp. 1065–1075, 1071; Loudon, op. cit. (1), pp. 96–97.

5 Friedrich Wieger, ‘Des moyens prophylactiques mis en usage au grand hôpital de Vienne contre l’apparition de fièvre puerpérale’, *Gazette médicale de Strasbourg* (1849) 9, pp. 397–405; editorial, *Gazette médicale de Paris* (1850) 3(5), pp. 382–383, available at [www.biusante.parisdescartes.fr/histmed/medica/page?90182x1850x05&cp=382](http://www.biusante.parisdescartes.fr/histmed/medica/page?90182x1850x05&cp=382).

the first maternity clinic of the Vienna General Hospital (Allgemeines Krankenhaus –AKH). Wiegner was thus an eyewitness to the new practice of chlorine hand disinfection, and his article was the first to publish the actual results of Semmelweis's experiment. This represented the strongest evidence Semmelweis had to prove his theory of the cause of childbed fever. An earlier editorial announcing Semmelweis's discovery, published in December 1847 by the Austrian physician Ferdinand Ritter von Hebra, had not actually published Semmelweis's results; it merely compared two unrepresentative averages to make it appear that hand disinfection had reduced the MMR in the first maternity clinic from 18 per cent to 2.45 per cent.<sup>6</sup> There was no known response to this editorial. Another one, published in April 1848, compared Semmelweis's discovery to Edward Jenner's discovery of the smallpox vaccine, but published no additional data.<sup>7</sup> There was, again, no known response to the second editorial, notwithstanding that both editorials requested 'directors of obstetrical institutions' to report 'confirming or disconfirming results to the editors of this journal'.<sup>8</sup>

Wiegner's article not only mentioned other sources of infection besides cadavers (as did Hebra's earlier editorial), but also described a case in which a midwife had infected many women 'by using one single, badly cleaned sponge to clean the genitalia of women', and thereby confirmed Semmelweis's conclusion that the causative agent could come from sources other than cadavers.<sup>9</sup> But Wiegner did more: he published for the first time the MMR in the first maternity clinic of the AKH for the years 1840–1848. These statistics showed that, each year, between 238 and 521 women had died of childbed fever in the first maternity clinic before chlorine hand disinfection was introduced, whereas only forty-five women died of childbed fever in 1848, the first full year after chlorine hand disinfection was implemented. Nevertheless, an editorial in the *Gazette médicale de Paris* rejected this evidence as proof that hand disinfection had caused the reduction in mortality, for the following reason:

We do not wish to assert that the doctrine derived from these facts is intrinsically and completely erroneous; we simply believe that this doctrine does not follow logically from these facts; for example, the remarkable fall *over a year and a half*, starting from when the hand washing measure was put into practice, could be due to a completely different circumstance. Who is not familiar with the capricious and singular fluctuations of epidemics, and especially of epidemics of puerperal fever?<sup>10</sup>

In other words, it did not follow logically from the fact that the MMR fell after chlorine hand disinfection was implemented that hand disinfection had caused the fall, as this 'could be due to a completely different circumstance', especially as the incidence of

6 Semmelweis, op. cit. (2), pp. 561–562. The average MMR for April and May 1847 (which was 17.2 per cent or 96/558; see Semmelweis, op. cit. (2), Table XV, p. 389), was taken as representative of the MMR before chlorine hand disinfection was implemented notwithstanding that the MMRs for the previous three months, January, February and March 1847, were 3.21 per cent, 1.02 per cent and 3.6 per cent respectively, and that the average MMR for the months June–November 1847, taken to represent the MMR after hand disinfection was implemented, obscured the outbreaks of childbed fever that had occurred in September, October and November 1847 (see Semmelweis, op. cit. (2), pp. 396–397).

7 Semmelweis, op. cit. (2), pp. 562–563.

8 Semmelweis, op. cit. (2), pp. 562–563.

9 Wiegner, op. cit. (5), p. 402.

10 Editorial, op. cit. (5), p. 383 (original emphasis).

childbed fever was known to fluctuate so widely: *post hoc* did not mean *propter hoc*. This previously unrecognized objection seems to have been the dominant objection to Semmelweis's proof, at least in Europe.

We have previously shown that Semmelweis's chief, Johann Klein, rejected Semmelweis's evidence that chlorine hand disinfection could prevent childbed fever for this same reason.<sup>11</sup> Evidence for this is contained in documents Erna Lesky discovered in the Austrian archives, and published in a monograph in 1964, but which had not previously been translated into English.<sup>12</sup> However, a strong hint that Semmelweis and the young doctors visiting Semmelweis were aware of Klein's objection is contained in a letter that Heinrich Hermann Schwartz, assistant to Gustav Adolph Michaelis, who was director of the maternity hospital in Kiel, wrote to Michaelis on 21 December 1849 summarizing Semmelweis's work, and which Carter and Tate have translated into English. In his letter, Schwarz concluded, about the effectiveness of chlorine hand disinfection, that 'while one dare not with certainty declare *propter hoc*, one can at least say *post hoc*, given the remarkable change that has occurred in the sanitary conditions of the institutions'.<sup>13</sup>

We have also shown that Semmelweis undertook experiments in domestic rabbits with Rokitsky's assistant, Georg Maria Lautner, specifically to rebut this very objection to his proof, and not, as Scholl has contended, to prove the mechanism of causation, i.e. pathogenesis, of childbed fever.<sup>14</sup> The purpose of these animal experiments was to prove that decaying animal-organic matter was not only a necessary cause of childbed fever, but could also be contingently a sufficient cause (contingent on there being a point of ingress for the decaying matter into the blood stream, and on the animal-organic matter having reached a sufficient stage of putrefaction).

Tulodziecki's contention that Semmelweis's 'final' theory of the cause of childbed fever was rejected because 'he had already, unsuccessfully, insisted twice before that he had identified the only cause of puerperal fever' has no basis whatsoever in facts.<sup>15</sup> Tulodziecki's claim that Semmelweis published three versions of his theory (which she called Versions 1, 2 and 3) was based on Loudon's misrepresentations of the sequence of events that followed the implementation of hand disinfection at the end of May 1847.

After Semmelweis deduced that the MMR on the first maternity clinic was so much higher than on the second (midwives') clinic because the causative agent was being transported from the morgue to the labour ward on the hands of attendants, he at first required attendants to disinfect their hands only before entering the maternity ward. Once they were on the ward he allowed them to wash their hands with soap and water between

11 Nicholas Kadar, 'A note on Semmelweis's animal experiments and their historical significance', *Journal of the History of Medicine and Allied Sciences* (in press).

12 Erna Lesky, *Ignaz Philipp Semmelweis und die Wiener Medizinische Schule*, Graz and Vienna: H. Böhlau, 1964, pp. 29–30, 46–47, §(f).

13 K. Codell Carter and George S. Tate, 'The earliest known account of Semmelweis's initiation of disinfection at Vienna's Allgemeines Krankenhaus', *Bulletin of the History of Medicine* (1991) 65, pp. 252–257, 256 (italics in original translation).

14 Kadar, op. cit. (11).

15 Tulodziecki, op. cit. (4), p. 1071.

examining women in labour as before. Although the MMR from childbed fever on the first maternity clinic fell immediately after hand disinfection was started, and remained low for three months, the MMR spiked again in October 1847 after a mother with an infected carcinoma of the uterus was admitted to the specific bed on the labor ward with which ward rounds always started. Eleven of the twelve mothers examined after this patient was examined developed childbed fever. Semmelweis quickly realized that purulent discharges could also be a source of the causative agent, and thereafter required attendants to disinfect their hands before examining any mother in labour.<sup>16</sup> However, the MMR spiked again the following month (November 1847) after a mother with an infected open wound of the knee was admitted. Semmelweis could tell by the odor in the room that ‘ichorous [purulent] exhalations [discharge] of the carious knee completely saturated the air of her ward’,<sup>17</sup> and concluded that in these limited cases – not *tout court* as under the miasmatic theory of epidemics, and as some historians have incorrectly believed – the cause of childbed fever could be construed as a miasma, and that such patients needed to be isolated.<sup>18</sup> But Semmelweis never published interim conclusions, as it were, and certainly never published an account of his theory stating that the causative agent could only come from cadavers (which Tulodziecki called Version 1 of his theory), and his theory of what caused childbed fever never changed.

According to Semmelweis, childbed fever was a form of pyaemia caused by the absorption of decaying animal-organic matter into the blood stream through the placental bed or injuries to the birth canal sustained during delivery, which was followed by alterations of the blood (which Semmelweis referred to as ‘disintegration of the blood’), and, in most cases, the anatomical changes observed at autopsy.<sup>19</sup> Semmelweis did identify cadavers as a source of the causative agent first, but he quickly realized that the causative agent could also come from living persons with purulent (ichorus) discharges as well as from cadavers, and that it could additionally be carried by air if patients with open infections caused the air in the ward to become sufficiently saturated with the causative agent.<sup>20</sup> Finally, Semmelweis realized that decaying matter could also originate in the mother’s own genital tract if decidual or placental remnants decompose before being extruded from the genital tract, and cause what he called ‘auto-infection’. Tulodziecki referred this source of the causative agent as Version 2 of Semmelweis’s theory, which she claimed Semmelweis had reported before the final version, i.e. Version 3, of his theory.<sup>21</sup> However, Semmelweis described ‘autoinfections’ in his lecture to the Society of Physicians on 15 May 1850, his first public account of his discovery, and not before anyone had reported Version 3 of his theory.<sup>22</sup> Thus

16 Semmelweis, op. cit. (2), p. 396.

17 Semmelweis, op. cit. (2), pp. 396–397.

18 Semmelweis, op. cit. (2), pp. 542–543; David Wootton, *Doctors Doing Harm since Hippocrates*, Oxford: Oxford University Press, 2006, p. 222.

19 Semmelweis, op. cit. (2), pp. 438, 504, 597.

20 Semmelweis, op. cit. (2), pp. 396–397.

21 Tulodziecki, op. cit. (4), p. 1074.

22 Tiberius von Györy, *Semmelweis’ Gassemete Werke*, p. 49, available online at <https://ia800300.us.archive.org/13/items/semmelweisgesamm00semm/semmelweisgesamm00semm.pdf>.

Tulodziecki's contentions about why Semmelweis's theory was rejected are based entirely on erroneous beliefs about what Semmelweis's theory actually was, and on how his theory had been communicated to the medical community.

Wieger's article contains other items of significant historical interest. First, Wieger's first-hand account of how chlorine hand disinfection was practised stressed the requirement for the attendants meticulously to scrub their fingernails with a brush.<sup>23</sup> Second, it contains entirely new information about a remedial measure the authorities had implemented to combat the very high mortality rate in the first maternity clinic before Semmelweis became the assistant: 'the wards of the two services were exchanged' but without effect, as 'deaths followed the first clinic into the ward of the second!'<sup>24</sup>

Finally, Wieger stated that he had conducted experiments in domestic rabbits, but was unable to induce infection in the animals.<sup>25</sup> He did not give any details of the experiments, except to say they were conducted in pregnant and recently delivered rabbits.<sup>26</sup> But what is so interesting about these experiments is not their details but the fact that Wieger felt it necessary to conduct animal experiments at all, given his statement that he was absolutely convinced about the correctness of Semmelweis's theory.<sup>27</sup>

It seems to us unlikely that Wieger and Semmelweis conducted animal experiments at about the same time purely coincidentally, and independently of each other, given that Wieger spent time with Semmelweis in Vienna, was convinced that Semmelweis's theory was correct, regarded Semmelweis as more authoritative than himself, and was in communication with Semmelweis.<sup>28</sup> It seems much more likely that Wieger conducted these experiments after discussing them with Semmelweis, and for the same reasons as Semmelweis conducted them, and perhaps even at Semmelweis's request, given that Semmelweis was at first prevented from conducting the experiments by Lautner's arrest following his participation in the October riots.<sup>29</sup>

## Conclusion

The importance of understanding what immediate opposition Semmelweis's theory faced from his contemporaries cannot be overstated for it is the starting point of a cascade of related events that explain what happened to Semmelweis in Vienna, and many of his previously unexplained actions. Considered jointly, these two documents provide the clearest and most direct evidence of the nature of that opposition, and explain why Semmelweis conducted animal experiments instead of publishing his results when he had sufficient evidence that chlorine hand disinfection was effective. Semmelweis's animal experiments in turn provide insights into the depth of Semmelweis's thinking that led to his discovery, and that the surgeon and medical

23 Wieger, *op. cit.* (5), p. 401.

24 Wieger, *op. cit.* (5), p. 400.

25 Wieger, *op. cit.* (5), p. 401.

26 Wieger, *op. cit.* (5), p. 401.

27 Wieger, *op. cit.* (5), p. 400.

28 Wieger, *op. cit.* (5), p. 398.

29 Lesky, *op. cit.* (12), p. 80.

historian Owen Wangensteen has described as ‘an example of penetrating insight without parallel in the history of medicine’.<sup>30</sup>

### Supplementary material

The supplementary material for this article can be found at <https://doi.org/10.1017/S0007087420000229>.

30 Owen H. Wangensteen, ‘Nineteenth century wound management of the parturient uterus and compound fracture: the Semmelweis–Lister priority controversy’, *Bulletin of the New York Academy of Medicine* (1970) 46, pp. 565–596, 566.