

# *Oxfordian Theory, Continental Drift* and the Importance of Methodology

By James A. Warren

**M**uch can be learned about why literary scholars have not accepted Oxfordian theory by comparing it with another theory introduced around the same time, continental drift. Admittedly the idea that the literary works traditionally attributed to William Shakespeare were actually written by Edward de Vere, Earl of Oxford, appears to have little in common with the idea that the major features of the earth's crust were formed by movement of its continents in the past. A close comparison of the two theories, however, reveals important reasons why literary scholars continue to reject the idea of de Vere's authorship of Shakespeare's works even though scientists have accepted continental drift as fact.

This paper is an examination of the two most important reasons: the incomplete nature of Oxfordian theory itself, and the prevalence in academia of a methodology for literary studies that is unreceptive to consideration of the Shakespeare authorship question. It is not intended to be a full statement of Oxfordian theory, its development or its reception by the academic community over the past century. But I believe the two factors discussed here will form important parts of any comprehensive history of the Oxfordian movement once one is written.

## **Similarities Between Continental Drift and Oxfordian Theory**

Around 1920, two innovative thinkers proposed highly radical theories that directly challenged existing explanations for phenomena in their respective fields. Alfred Wegener proposed the idea that the major features of the earth's geography – its continents and oceans – had changed shape, size and location over time, a theory that became known as continental drift. Also, in that year, J. Thomas Looney proposed that the literary works attributed to William Shakspere of Stratford-upon-Avon had actually been written by Edward de Vere, Earl of Oxford.

The two theories were examined by their respective intellectual communities of scientists and literary scholars in the 1920s, and both were adamantly rejected. Forty years later, in the 1960s, continental drift was reborn under the name plate tectonics, "heralded as a major scientific breakthrough . . . and established as scientific fact."<sup>1</sup> Oxfordian theory, however, was not resurrected in the 1960s. Even after an additional

fifty years, it has not been accepted by the majority of literary scholars, who continue to teach their students that Shakespeare's works were written by the man from Stratford.

Both new theories were attempts to explain anomalies in existing theories. In the case of continental drift, the reigning belief among American geologists was permanence theory, the idea that the earth's features had always been the size, shape and location they are today. That theory, however, was unable to explain the origin of mountains, the complementary jigsaw puzzle shapes of the continents or similarities in the flora, fauna and rock formations on continents thousands of miles apart.

Wegener, a German geophysicist, argued in *The Origin of Continents and Oceans* (1915, 1928) that if the continents had moved over time, many of the largest problems of earth history would be solved. As Naomi Oreskes observes in her study of continental drift, the movement and resulting interaction of the continents would explain the existence of mountain chains and "resolve the seemingly conflicting data of geophysics and paleontology."<sup>22</sup>

Regarding the authorship of Shakespeare's works, by 1920 the suspicion that they could not have been written by the man from Stratford had been growing for more than fifty years as the disconnect between the biographical details of his life and the qualities, experiences and types of knowledge that the author must have had in order to write the works became known.

J. Thomas Looney, a Durham county schoolmaster, sought to find the real author, and in




---

**James Warren** was a Foreign Service officer with the U.S. Department of State for more than twenty years, during which he served in public diplomacy positions at U.S. embassies and consulates in eight countries, mostly in Asia. Following his career with the State Department he served as Executive Director of The Association for Diplomatic Studies and Training (ADST) and then as Regional Director for Southeast Asia for the Institute of International Education (IIE). James is currently devoting his energies to literary pursuits, and in 2013 received the *Vero Nihil Verius* Award for Distinguished Shakespearean Scholarship from Concordia University for his accomplishment as the editor of *An Index to Oxfordian Publications*. He is a Fellow with the Center for the Study of the Great Ideas and the Adler-Aquinas Institute.



*"Shakespeare" Identified* (1920) he described how his systematic search revealed "a most exceptional set of resemblances"<sup>23</sup> between the qualities the author had to have had and only one person living at the time Shakespeare's works were written: Edward de Vere. If de Vere is accepted as Shakespeare, Looney wrote, "the accumulation and combination of anomalies"<sup>24</sup> arising from authorship by Shakespeare that had haunted literary studies for more than half a century melts away.

Both new theories were supported by circumstantial evidence. Continental drift was supported by similarities between flora and fauna on continents thousands of miles apart. By the 1920s, paleontologists had established 57 flora and fauna similarities between Australia and India, Africa and Brazil, Madagascar and India, and Europe and North America, and geologists had documented extensive similarities between fossil records and rock formations.

Oxfordian theory was likewise supported by a large number of similarities, in this case between events and people important in the life of Edward de Vere on one hand, and events and characters in Shakespeare's plays on the other. One notable example was the Gad's Hill robbery perpetrated by servants of Edward de Vere in real life and portrayed in *Henry IV, Part 1*. Early researchers such as Eva Turner Clark documented scores of other similarities between events depicted in the plays and events in de Vere's life and in Elizabeth's court and government that took place fifteen years too early for the man from Stratford to have been the author. As Looney comments on this point, "It is because the Shakespeare literature embodies work representing all periods of Oxford's lifetime, sometimes in a single play, that efforts to fix a Shakespeare canon on the basis of an author younger than the Earl of Oxford have proved so inconclusive."<sup>25</sup>

To Wegener and Looney, the large number of coincidences proved their cases. Wegener believed that "Taken individually, any one of these matches might be dismissed as coincidence, but the totality of these points of correspondence constitutes an almost incontrovertible proof."<sup>26</sup> Looney similarly explained that "The predominating element in what we call circumstantial evidence is that of coincidences. A few coincidences we may treat as simply interesting; a number of coincidences we regard as remarkable; a vast accumulation of extraordinary coincidences we accept as conclusive proof."<sup>27</sup>

Although both new theories explained anomalies arising from existing theories, both were incomplete in a major way – a way similar to both – that enabled believers in the older theories to cling to them.

The principal weakness of continental drift theory was the lack of a causal mechanics to explain continental movement. Wegener's theory did not identify a force strong enough to push continents through the rigid ocean floor nor a reason why such a force, if one existed, would actually do so.

Oxfordian theory suffered from similar failures to identify a motive (why) and a mechanics (how). Looney's original theory did not explain why Edward de Vere would have wanted to hide authorship of his plays and poems. He purposefully did not question de Vere's motive for hiding his works, explaining that:

It is made as clear as anything can be that he [de Vere] . . . had elected his own self-effacement, and that disrepute was one, if not the principal, motive. We may, if we wish, question the sufficiency or reasonableness of the motive. That, however, is his business, not ours. The important point for us is that he has by his sonnets disclosed the fact that he, "Shakespeare," was one who was concealing his real name, and that the motive he gives, adequate or not, is one which unmistakably would apply to the Earl of Oxford.<sup>8</sup>

Again, "When, therefore, he [de Vere] tells us, in so many words, that "vulgar scandal" had robbed him of his good name, and that although he believed his work would be immortal he wished his name to be forgotten, we are quite entitled to take his own word for it, and to demand no further motive for the adoption of a disguise."<sup>9</sup>

And yet, this is insufficient. The extraordinary nature of the works demands a fuller explanation for the author's motives in hiding his authorship, and for why other people during de Vere's lifetime and for decades after his death would have wanted his authorship hidden.

Looney also did not explain the mechanics of how the effort to hide de Vere's authorship could have been accomplished, given the number of people who would have been aware of it. Charlton Ogburn comments that the extraordinary effort that would have been needed to hide Oxford's authorship was "highly implausible" and "its implausibility is what has chiefly blocked a more general acceptance of "Shakespeare" as having been a pseudonym."<sup>10</sup>

If the lack of a mechanics and motive were the major flaws with both new theories, they were also flaws in the older theories. Permanence theory could not explain how it was that similar flora, fauna, rock formations and fossils existed on continents thousands of miles apart. Stratfordian theory could not explain how an uneducated

provincial could have acquired the depth of knowledge in so many areas needed to write the learned plays.

Thus, all four theories, old and new, were incomplete in major ways. It is not sufficient merely to say that these four things happened. Nor is it sufficient to wave a magic wand and say that the continents moved, or to chant “genius” to explain how the man from Stratford acquired the extensive knowledge in so many areas exhibited in the plays. Given these weaknesses, it was a toss-up at the time as to which of each pair was correct.

The factors determining which theories would be accepted were the framework and methodology dominant in the respective scientific and academic communities at the time the theories were introduced.

### The Scientific Environment for Continental Drift

Both new theories were initially received with interest by professionals in their respective fields. Regarding continental drift, British geologist Philip Lake expressed the surprise he and his colleagues felt: “A moving continent is as strange to us as a moving earth was to our ancestors, and we may be as prejudiced as they were,” but he also recognized that “if continents have moved, many former difficulties disappear.”<sup>11</sup> On the Shakespeare authorship side, literary scholar D. Willoughby similarly recognized that “Half the most baffling Shakespearean riddles could be answered by assuming that Lord Oxford was the author.”<sup>12</sup>

Scientists and literary scholars soon moved beyond their initial surprise to examine the theories more closely. They did so within the frameworks or structures already in place in their respective intellectual communities. Those frameworks included a guiding idea, “facts” already believed to be true, and a methodology believed to be the correct process through which new knowledge in their field could be uncovered. The guiding idea defined the major task to be carried out by the scientists or literary scholars and steered them toward fruitful areas of investigation. The existence of facts already known, together with the principle of coherence, meant that new data was suspect if it conflicted with existing facts.

Methodology was the most critical of the three factors in the frameworks because it determined that new theories would be suspect if they were not formed in accordance with the existing methodological process. Naomi Oreskes, author of *The Rejection of Continental Drift*, recognizes the key point that:

Science – the search for truth – is not about belief; it is about how belief gets formulated. . . . At any given moment, only a finite set of knowledge satisfies the reigning criteria for the formulation of scientific belief, and only this knowledge is eligible as truth.<sup>13</sup>

In comments equally applicable to literary scholars, she observes that scientists are “constantly making choices: about the questions they pursued, about the methods they used to answer them, and about the ways in which they interpreted and presented their results.”<sup>14</sup> These choices are made within the internal cultures of each discipline, cultures that “are, at least in part, constituted in advance of any given theoretical debate.”<sup>15</sup> Oreskes also notes a point critical for acceptance or rejection of both theories: that “internal cultures” and methodologies change over time. As she explains, “[T]he discriminating criteria are historically contingent; over time and across communities, they shift, they evolve, they are overthrown, they transmute.”<sup>16</sup>

At the time Wegener introduced the theory of continental drift, the guiding idea in the American geologists’ framework was that of permanence theory or uniformitarianism. That assumption provided the context for their work and influenced the theories they formulated to explain the origin of the geological formations they studied. They subscribed to that principle, Oreskes explains, “because it was enabling. It enabled them to interpret field evidence in a consistent and logical way. It enabled them to build a science of the past that would otherwise have remained logically inaccessible.”<sup>17</sup>

Equally important was American geologists’ adherence to the strictly inductive methodology that had resulted from their experiences exploring an enormous continent over more than a century. Their methodology was that of the field scientist, and consisted of travelling to the sites of rock formations, outcroppings and other geological features to map, study, and classify them.

Because so little was known about North America’s geology when that long-term effort to explore it began, geologists believed it was good scientific practice to conduct their investigations without preconceived explanations for that they might find. One historian notes that “With a vast, largely undefiled geological laboratory stretching before them, American geologists devoted themselves to exploration and observation rather than to speculation and to theory building.”<sup>18</sup> Another observer described their insistence of keeping “explanations for what they observed . . . clearly separate from the facts. Only after such appraisal did one know what was in need of explanation.”<sup>19</sup>

American geologists believed that adherence to an inductive methodology was necessary to defend against the natural human tendency to seek support for theories already held, and to reject evidence that contradicts them. Because this point has great relevance for the reception of both new theories that this paper considers, it is worth pausing to note geologist T. C. Chamberlin’s justification for his field’s methodology.

Once any theory is held in a preferred position . . . There is the imminent danger of an unconscious selection and a magnifying of phenomena that fall into harmony with the theory and support it, and an unconscious neglect

of phenomena that fail of coincidence. . . . Instinctively, there is a special searching-out of phenomena that support it. . . . the mind rapidly degenerates into the partiality of paternalism. The search for facts, the observation of phenomena and their interpretation, are all dominated by affection for the favored theory until it appears to its author or its advocate to have been overwhelmingly established. . . . a premature explanation passes first into a tentative theory, then into an adopted theory, and lastly into a ruling theory.<sup>20</sup>

A methodology in which general theories were formed only after extensive geological facts were obtained through field work was perhaps an appropriate methodology for geologists exploring a new continent.

Wegener's theory of continental drift came as something of a shock to American geologists, and reaction to it was "almost entirely negative."<sup>21</sup> They were predisposed to reject it not only because it violated their guiding principle of permanence theory, but more importantly because the process through which Wegener formulated and promoted his theory violated practically every aspect of the American geologists' methodology. Oreskes explains that

One can see why Americans so reacted to Alfred Wegener's argument for continental drift. In content, in manner, and in purpose, Wegener's work contradicted the edifice and rhetoric of practice that Americans had laboriously constructed and articulated over the course of nearly a century. . . . The theory of continental drift was universalist and comprehensive, it was presented as the result of sudden inspiration rather than long labor, and the format in which he presented it violated the American pattern of putting facts first.<sup>22</sup>

American geologists believed that Wegener's having put theory first and then seeking facts to support it was bad scientific practice. One prominent American geologist, Bailey Willis, felt that Wegener's book gave the impression of having been "written by an advocate rather than an impartial investigator."<sup>23</sup> Americans were further incensed because Wegener, in his own words, had hit on the idea "by accident" and through "hasty analysis" rather than through the hard work of field investigations that geologists are supposed to engage in.

Wegener . . . could hardly have said anything more likely to inspire American indignation. For Americans, coming to an important idea "by accident" looked like a summary dismissal of the role of hard work in the formation of reliable scientific knowledge. Wegener's [method] . . . looked like Joseph Seingewald's "selective search through the literature for corroborative evidence."<sup>24</sup> Chamberlin too had explicitly warned against the unconscious selection of facts that fit preconceived theories. And Wegener freely admitted to conscious selection of such facts!<sup>25</sup>

## The Academic Environment for Oxfordian Theory

Looney introduced the idea of Edward de Vere's authorship of Shakespeare's works into a more receptive intellectual environment than did Wegener when he introduced continental drift. The guiding idea in Shakespeare studies at the time was that the works had been written by William Shakspere of Stratford, but, as noted, doubts about his authorship had been in the air for more than half a century.

"The undermining of that belief," Looney explains, was due "mainly to two movements . . . [arising in] the nineteenth century." The first was the marked interest in practical historical research, which "brought to light the disconcerting fact that the English writer most distinguished by the brilliancy of his powers was, paradoxically, separated from all his fellows by a glaring deficiency of relevant personal records." The second was the development of a scientific study of literature, which "yielded a truer measure of the culture represented by the works." These two developments "produced in many minds a definite conviction that . . . a school of literature of the first rank had been allowed to grow up around a personality having no title whatever to the honour."<sup>26</sup>

These doubts penetrated less deeply into the academic/scholarly community than in the wider cultural world though, and authorship by the man from Stratford remained the guiding idea in academia. Given the "facts" already known to them – that Shakespeare's authorship had been confirmed by the First Folio and that the plays had been written for the public stage – scholars' efforts were focused on fleshing out their understanding of the context in which the works had been written, with that context defined by the timeline of Shakspere's life.

Stratfordians, then, in contrast to the American geologists, were guided by a methodology that could be described as deductive. Just how limiting this approach was is shown by the case of Charlotte Carmichael Stopes, who spent decades searching for evidence of ties between William Shakspere and the Third Earl of Southampton, the dedicatee of Shakespeare's two long poems. Unable to find even a single scrap of evidence to connect the two men, she regarded her search as a failure. With a more open-ended methodology, she might have come to realize that her assumption of Shakspere's authorship was mistaken.

For Stratfordians, authorship by other candidates could not possibly be correct regardless of the lack of correspondences between Shakspere and the works and regardless of the number of coincidences uncovered between events in the plays and events in the lives of other purported authors, just as for adherents to permanence theory continental drift could not have occurred regardless of the number of similarities in flora and fauna found on continents thousands of miles apart. With correspondences between the life and works ruled out as an acceptable form of evidence by their methodology, Shakespearean scholars felt justified in concluding that

insufficient evidence existed to justify academic consideration of the Shakespeare authorship question.

Looney's methodology was similar to that of the American geologists in that he sought to investigate the authorship question guided only by qualities he thought the author must have had and without prejudging what he might find. In approaching the authorship question in this manner, Looney was acting in the role of an investigator. Because what he was investigating took place in the past, he was in effect conducting the work of a historian. It is appropriate, then, to consider the methodology most appropriate for historians.

"History," writes noted historian David Hackett Fischer, "must begin with questions. Questions for historians are like hypotheses for scientists."<sup>27</sup> In asking an open-ended question and in presenting his results "in the form of a reasoned argument," Looney seems almost to be following the process of "adductive reasoning" that Fischer describes fifty years later as most appropriate for historians.

Once Looney had discovered de Vere authorship, Oxfordians began to follow a process resembling the methodology of the Stratfordians. Both sought to establish the facts of "their" candidate's life, and both also employed Fischer's adductive process as they sought to write coherent accounts of how their candidate had come to write his works.

In sum, at the time Looney introduced the idea of de Vere as Shakespeare, Shakespeare studies were characterized by the idea of authorship by the man from Stratford within academia and growing doubts about his authorship outside it.

## The Two New Theories Are Rejected

Proponents of both older theories tried to explain away the anomalies their theories could not account for, in part by resorting to ad hoc explanations. Geologists who held to permanence theory had somehow to account for the similarities in flora, fauna and fossil records found on continents thousands of miles apart. To do this, they proposed the ad hoc idea of sunken continents – land masses that had once connected continents existing today, but which had sunk after having served as transit territory for the flora and fauna.

One such continent was supposedly located between what are today India and the island of Madagascar. Because lemurs are found in both India and Madagascar – and only in those two places – and because the lemurs are too similar to have evolved independently, the British zoologist Philip Sclater postulated that a now-sunken continent that he called Lemuria had once connected the two places.<sup>28</sup>

Stratfordians also invented ad hoc explanations for things that otherwise could not

be explained in any rational way if the author was born in Stratford in 1564. As one example, when evidence arose that a play with a character named Hamlet had existed by the end of the 1580s, far too early to have been written by Shakspeare, they fantasized about the existence of an anonymous play they called ur-Hamlet on which Shakespeare based his play. Since the ur-Hamlet play no longer exists, perhaps it too, like Lemuria, has sunk in the Indian Ocean.

After sunken continents were shown to have been impossible, prominent geologists such as Bailey Willis and Charles Schuchert launched a new ad hoc idea, that of intermittent land bridges to explain how flora and fauna could be so similar on continents so far apart, even though “no independent evidence” supports the idea “that the postulated land bridges ever existed.”<sup>29</sup>

What is so odd about these episodes is that American geologists – the very same individuals – who had objected so strongly to the speculative nature of Wegener’s theory were now engaging in speculation of their own. Their fantasies about sunken continents and intermittent land bridges show just how powerful the desire to justify beliefs already held can be, and perhaps how justified American geologists had been in adopting such an extremely inductive methodology to defend against that temptation. This episode should also show Oxfordians just how strongly Stratfordians can be expected to continue to defend their existing belief in authorship by the man from Stratford.

The two new theories were also both subjected to unscientific and un-academic attacks by scientists and scholars. Critics of continental drift, for example, continued to attack an outdated version of the theory from the early 1920s rather than the more sophisticated version published in 1928. In 1943, paleontologist George Gaylord Simpson “framed his response on the now anachronistic 1924 Skerl translation of Wegener and ignored du Toit’s more recent first-hand work.”<sup>30</sup> Critics of the idea of de Vere’s authorship followed a similar practice, often criticizing the ideas of Delia Bacon from the 1850s, rather than addressing the most sophisticated evidence in support of de Vere’s authorship presented by Charlton Ogburn and others.

Opponents also used spurious arguments against both theories. George Gaylord Simpson, again, argued “that evidence from mammalian evolution did not support drift” at a time when it was already widely accepted that mammals evolved after the time when continental drift had separated most of the continents.<sup>31</sup> Similarly, Stratfordians repeatedly cited the “fact” that de Vere could not have written many of the plays because they had been written after his death in 1604, while knowing full well that the date of composition has not been established for any of the plays.

Supported by fantasies and ad hoc explanations to explain anomalies and by unscientific and un-academic attacks on the new theories, the older theories remained the accepted explanations in their respective intellectual communities.

Regarding continental drift, Oreskes notes that “If the geologists did not agree to a man to accept isthmian links, many of them did.” With “faunal homologies . . . removed from the list of arguments in favor of drift,” because they could be explained by land bridges, “Wegener’s argument was drastically undermined.”<sup>32</sup> Acceptance of land bridges “effectively marked the end of active debate over continental drift in the United States.”<sup>33</sup> The theory was “officially rejected by the influential American Association of Petroleum Geologists”<sup>34</sup> in 1928, and from that point on, “for the better part of three decades, American geology students were taught that flora and fauna had migrated among ancient continents via narrow, intermittently emergent land bridges.”<sup>35</sup>

Observing this situation, Oreskes concludes that continental drift theory was rejected by American geologists not because it lacked an explanation for the mechanics of continental movement, but because Wegener’s methodology conflicted so drastically with what they believed was good scientific practice.

American earth scientists rejected the theory of continental drift not because there was no evidence to support it (there was ample), nor because the scientists who supported it were cranks (they were not), but because the theory, as widely interpreted, violated deeply held methodological beliefs and valued forms of scientific practice. The idea of the motion of continents, the empirical evidence for it, and the mechanical explanations of it . . . have all been corroborated by contemporary earth science. But to accept all these ideas in the 1920s or early 1930s would have forced American geologists to abandon many fundamental aspects of the way *they did* science. This they were not willing to do.<sup>36</sup>

To cite two examples in support of this idea, geologist Rollin Chamberlin noted in 1928 that “if continental drift were true, geologists would have to forget everything which has been learned in the last 70 years and start all over again.”<sup>37</sup> “Very naturally,” geologist Chester Longwell explained, “we insist on testing this hypothesis with exceptional severity, for its acceptance would necessitate the discarding of theories held so long that they have become almost an integral part of our science.”<sup>38</sup> Stratfordians today surely have similar sentiments.

American geologists sought to protect themselves from bias in favor of data supporting existing theories by adhering to an extremely inductive methodology. Yet ironically it was the strength of their adherence to that methodology that led them into a related error – that of rejecting another theory (not other data) because it had been formulated through a methodology that was believed to be flawed. As we will see, the reluctance to abandon long-held beliefs and methodologies – and to blindly reject theories formed and facts discovered under different methodologies – are factors with direct relevance to Stratfordians and cultural theorists in academia today.

Turning to the authorship question, although the weight of academic opinion was opposed to the idea that William Shakespeare was a pseudonym behind which lay the pen of Edward de Vere, some scholars remained open to it. Henry Clay Folger, founder of the Folger Shakespeare Library, was so intrigued by Esther Singleton's novel *Shakespearian Fantasias* (1930), in which characters from Shakespeare's plays quote poems by Edward de Vere and describe other characters using words that were actually used to describe de Vere, that he purchased a dozen copies and sent them out to major players in the field of Shakespearean research. He also purchased the original manuscript, which is now part of the Folger Library's collection.

Although Oxfordian theory was not officially rejected by an academic body in the 1920s as continental drift had been, it was rejected just as definitively. Most scholars did not take the authorship question seriously because for them Shakespeare's authorship had been confirmed by statements in the First Folio, and many were not even aware that de Vere had been proposed as a candidate for authorship.

Statements by directors of the Folger Library who succeeded Henry Folger are indicative of how the authorship issue was viewed within academia. Louis B. Wright, Director of the Folger Library, characterized those who doubt authorship by the man from Stratford as

‘disciples of cults’ that ‘have all the fervor of religion,’ prey to ‘emotion that sweeps aside the intellectual appraisal of facts, chronology and the laws of evidence.’ They are ‘fantastic sectarians’ who ‘rail on disbelievers and condemn other cultists as fools and knaves,’ and ‘who welcome a new convert to their beliefs with the enthusiasm accorded a repentant sinner at a Holy Rollers’ revival,’ while ‘a fog of gloom envelops them.’ They have developed a ‘neurosis . . . that may account for an unhappy truculence that sometimes makes them unwelcome in polite company.’ Indeed, ‘one gets the impression that they would gladly restore the faggot and the stake for infidels from their particular orthodoxy.’<sup>39</sup>

Showing just how little has changed within academia since Wright published those comments in the *The Virginia Quarterly Review* (VQR) in 1959, the VQR selected Wright's article as one of only four from the 1950s included in *We Write for Our Own Time: Selected Essays from 75 Years of The Virginia Quarterly Review*,<sup>40</sup> published in 2000. Given the viciousness of the characterizations of those doubting authorship by the man from Stratford by Wright and other leading academics, it is not surprising that almost all English professors continue to teach their students that Shakespeare wrote Shakespeare's works whether they believe that to be the case or not.

## Continental Drift Completed and Accepted

Development of the two theories continued under the radar for several decades after they were rejected. Then, in the 1960s and 1970s, their paths diverged. The scientific community accepted the idea of continental movement but literary scholars continued to reject the idea that de Vere had written Shakespeare's works. One key reason for the different outcomes was differences in the progress of the theories' development.

Even before continental drift had been formally rejected, technological advances outside geology – such as the harnessing of electricity and the invention of the internal combustion engine – began to give geologists new technical capabilities for examining geological formations. New tools, including sonar developed during the Second World War, had enabled oceanographers to map the sea bed for the first time, revealing the existence of the mid-Atlantic and Java trenches and chains of volcanoes running through the centers of several other oceans. Other tools enabled scientists to determine that the sea floor was expanding or spreading on either side of the undersea mountain chains. Still others, such as David Christian, established the key point that “the uppermost layer of the earth . . . consists of a number of rigid plates, like a cracked eggshell. . . . [which] move over a layer of softer materials just below them.”<sup>41</sup>

American geologist Harry Hess, in 1962, was the first person to begin to put these and other facts together in a coherent explanation of the mechanics of continental movement. That explanation, which became known as plate tectonics, describes how

Lava, seeping up through cracks that ran through most of the major ocean systems, was creating new seafloor. . . . As new oceanic crust was formed, it reared up in huge ridges of basalt . . . [that] acted like a wedge, driving apart seafloor that already existed. As a result, some oceans, such as the Atlantic, appeared to be widening.<sup>42</sup> . . . In other words, it is the heat of the earth's interior that provides the power needed to move great plates of matter about the surface of the earth.<sup>43</sup>

Plate tectonics thus resolved several conflicts that had blocked acceptance of continental drift. Continents did not need to push their way through rigid ocean seafloors, nor did they drift at random like icebergs. Rather, plates containing both continents and oceans were pushed apart by forces within the earth and carried by convection currents in the heavier but softer material on which the plates rested. One of the principal places of seafloor spreading was the Java Trench in the Indian Ocean, which split what is now Madagascar, with its lemurs, toward the west, and what is now India, with its lemurs, to the east.

Here, then, was an explanation of the mechanics powerful enough to move

continents and to fill the hole that had existed at the core of the theory of continental drift.

Oxfordian theory also continued to develop after it was rejected by academia in the 1920s. One line of research has documented just how doubtful the evidence supporting authorship by the man from Stratford really is. Another line has established the facts of Edward de Vere's life and the tightness of the correspondences between it and Shakespeare's works. And yet, even with significant advances in knowledge in those two areas, the academic community remains firm in its belief that the man from Stratford wrote Shakespeare's works and that de Vere did not.

Part of the justification for their belief is that the "how" and "why" questions that Looney pointedly did not address have not yet been definitively answered. The traditional explanation is that de Vere could not acknowledge authorship of his literary works because of his status as a courtier. In addition, he and others would have wanted his authorship hidden because of the portrayal and ridicule in his works of prominent personages in the court and government. Hiding his authorship would make identification of the people portrayed in them less likely.

There is a lot to be said for this explanation, and practically all Oxfordians agree that these factors play a significant role in the explanation for why de Vere's authorship was hidden. However, a substantial minority of Oxfordians believe that explanation is not emotionally weighty enough to account for the shame that de Vere repeatedly expresses in the Sonnets, or explain why the effort to hide his authorship continued for decades after the deaths of de Vere and those ridiculed in the plays. That explanation also does not adequately explain how such an extraordinary effort could have been carried out successfully. If they are right, the hole at the core of Oxfordian theory remains unfilled.

The incompleteness of Looney's theory can be compared not only with the incompleteness of Wegener's theory, as already noted, but also with Charles Darwin's theory of the origin of species through natural selection. All three theories generated widespread interest and comment when they were introduced, yet none was immediately accepted as fact in their original form because all were incomplete: all three lacked an explanation for the mechanics of how they worked.

Two of the theories – continental drift and evolution through natural selection – became widely accepted as fact only after a mechanics explaining their processes were discovered and developed to complete them. Continental movement, as noted, was not accepted until it was completed by the mechanics explained in plate tectonics in the 1970s. Darwin's theory of natural selection was not widely accepted as fact until the formulation of population genetics in the 1920s, which explained the mechanics of how traits selected by the environment were passed on to succeeding generations. The completed theory of evolution is now known as The New Synthesis.

Oxfordian theory remains unaccepted by academia in part because the second phase in the development of the theory has not yet taken place. Unlike plate tectonics and the New Synthesis, a gaping hole remains at the heart of Oxfordian theory because Oxfordians themselves have not yet united behind an explanation for the motives of those involved in hiding de Vere's authorship or a mechanics to explain how his authorship could have been successfully hidden.

Some of those not satisfied by the traditional explanation for why de Vere hid his authorship have proposed an alternative or supplementary explanation, the so-called Prince Tudor (PT) theory. Proponents of the theory believe that it is in accordance with the facts revealed in historical documents and by Oxford himself in his plays and poems, especially the Sonnets. Others, however, believe that this thesis is too speculative or simply incorrect.

The PT explanation centers around the idea that the effort to hide de Vere's authorship was related to his sexual involvement, in some manner, in the succession to Queen Elizabeth. If so, the sexual and even incestuous aspects of the situation would have been emotionally weighty enough to account for the shame that de Vere describes in the Sonnets. The potential threat to King James's reign by a natural descendant of Elizabeth sired by de Vere – the Earl of Southampton – would have provided strong political reasons for the use of state power to hide those facts throughout James's reign. That de Vere inserted veiled references to his liaison with the queen and the birth and status of Southampton into his plays and poems provides a motive for the use of state power to hide his authorship throughout the Jacobean/Stuart era. The case for this explanation has been made most powerfully by Hank Whittemore and Charles Beauclerk.<sup>44</sup>

At the Shakespeare Oxford Fellowship conference in 2014, I presented a paper on the use of state power to explain the mechanics of the effort to hide de Vere's authorship. In that presentation I urged the audience to keep an open mind about the Prince Tudor theory until a fuller examination of it has been undertaken. That effort has already begun, with Peter Rush's forthcoming book, *Hidden in Plain Sight* reaffirming and extending Whittemore's insights in *The Monument* that the Sonnets recount the story of the Earl of Southampton's imprisonment after the Essex Rebellion and provide justification for concluding that he was the son of Edward de Vere and the queen. The following table shows one way Oxfordian theory might be completed.

Issue under Examination	Initial Version of New Theory		Mechanics/Motivating Force Added Later		Complete Theory
Origin of species	Natural selection: Charles Darwin	+	Population genetics: Th. Dobzansky, G. Simpson, E. Mayr	=	New Synthesis
Features of earth's crust	Continental drift: Alfred Wegener	+	Sea floor spreading and movement of plates rather than continents per se: H. Hess, W. Menard	=	Plate Tectonics
Authorship of works attributed to Shakespeare	Edward de Vere as author: J. Thomas Looney	+	Veiled references to Southampton's royal origins in de Vere's works is the motive for hiding his authorship; use of state power explains the mechanics: Percy Allen, Hank Whittemore, others	=	Oxfordian Theory Completed

**Two phase introduction of three theories.**

We now turn to the second reason why continental drift has been accepted and Oxfordian theory hasn't: changes in the dominant methodologies in each area since the 1920s. In short, the methodology of geologists became more favorable to the acceptance of continental drift, while the methodology of literary criticism moved in a direction unfavorable to the Shakespeare authorship question.

A case will be made here that the critical event leading to acceptance of the idea of continental movement was not the formation of a mechanics that explained how that movement was possible but rather the change in methodology that preceded the formation of the mechanics. Neither the investigations that led to the formulation of the new theory nor geologists' acceptance of it would have been possible without the prior movement away from the overly restrictive inductive methodology that had led to rejection of Wegener's theory.

The new methodology was initially resisted by most geologists because they attributed their success in mapping the geological terrain of North America to their inductive methodology. Those few who supported the new practices faced the question of how to move to them before they had proved themselves, when the methodology that supported them had not yet been formulated, and when current methodology and bureaucratic pressures pushed against them.

Understanding the process through which geologists succeeded in breaking free of the older methodology and the bureaucratic supports for it is a story with some relevance for literary scholars who recognize the importance of academic consideration of the Shakespeare authorship question but who face methodological constraints and institutional pressures against it.

The new technical capabilities spurred a change from geology to geophysics, from a methodology of direct observation of geological formations in their original physical surroundings, to one emphasizing instrumental determinations and calculations of physical property of the earth in laboratories. This change was reflected in the type of data considered most relevant, the older being “observations described in words or pictures, the newer of numerical data,” and in the tools used. “The tools of the continental drift debate were hammers, hand-lenses, and field notebooks; those of the plate tectonics revolution seismographs, magnetometers, and computers,” according to Oreskes.<sup>45</sup>

Clashes occurred between those who were quicker to adopt the newer practices and those who adhered to the older methods. Oreskes notes that each group, “affirmed the values and strengths of its chosen methodological approach and implicitly or explicitly denied the values and strengths of others. Laboratory scientists promoted exactitude, precision, and control; field geologists promoted authenticity, accuracy, and completeness.”<sup>46</sup> Most geophysicists, she observes, “assumed that geologists were simply mistaken, while geologists either ignored geophysics or lived uncomfortably with the contradictions. As time went on, each side became increasingly frustrated with the other.”<sup>47</sup>

At the same time, as the value of the newer geophysical practices became better known, more geologists began to incorporate them into their own work. They did so, however, to reach goals they already held, including that of showing the impossibility of continental movement. In doing so they were, in their own eyes, not abandoning the older practices but merely adding more quantitative practice to it. As Oreskes recounts, geologists “had never argued for laboratory methods as a replacement for field geology. They saw it as a complement to it.”<sup>48</sup>

Yet that step of using the newer practices alongside the older proved fatal to the older methodology. At some point a line was crossed, and geologists began to give preference to data produced by the new geophysical practices even when it conflicted with data from their own field.

These changes in geologists’ practices mirrored the change in methodology occurring in the natural sciences more generally. As formulated by Karl Popper in the 1940s, science progresses through a series of “conjectures and refutations.”<sup>49</sup> Conjectures – scientific theories or informed guesses – are proposed, and attempts made to refute them. The more critical tests a theory passes, the more justified scientists are

in relying on it. In this methodology the place for intuitive leaps in thinking comes at the beginning of the process, not the end as in the geologists' inductive method. The geologists' fear that scientists would be tempted to cherry-pick data to prove theories formulated in advance of investigations is avoided by Popper's insistence that investigations should attempt to disprove conjectures rather than support them.

In sum, the older inductive methodology in which geologists' activities had been defined by visible geological outcrops, had been replaced by the newer deductive and theoretical methodology necessary to examine features often no longer visible with the naked eye. This change took place long before the formation of plate tectonics theory and was a necessary precursor to it.

Although most prominent geologists initially resisted changes to practices and methodology, the transition to them was helped immensely by a small number of senior geologists who publicly acknowledged that the older practices were outdated. The most prominent example is that of William Bowie, the leader of American geodesy and the namesake of the American Geophysical Union's annual William Bowie Medal. Bowie was "the man who saw the significance of these developments most clearly" and who recognized that "everything he had believed was being challenged."<sup>50</sup>

At the 1936 American Geological Union General Assembly, Bowie publicly stated that the Pratt model of isostasy – an idea that he had spent his career establishing as fact and that formed the single strongest piece of evidence cited against the possibility of continental movement – could no longer be regarded as proven. He acknowledged that "what he had 'proved' twenty-five years before was now being unproved," and he raised "once again the question of the Wegener hypothesis: . . . Ten years after he had called continental drift impossible, William Bowie now suggested" that not only was it possible, but that "the geodeists would be the ones to prove it."<sup>51</sup>

The courage and dedication to truth that Bowie showed in publicly acknowledging that his life's work had become outdated might serve as a model for senior Stratfordians today.

## Changes in the Methodology of Literary Criticism

It would be pleasing to say that literary scholars seriously investigated the merits of Edward de Vere's authorship of Shakespeare's works before rejecting the idea in the 1920s and 1930s. It would also be pleasing to describe how the methodology of literary criticism evolved in ways more favorable to the authorship question since then. Unfortunately, neither happened. Since the middle of the twentieth century the methodology of literary criticism has evolved in ways increasingly unreceptive to consideration of the Shakespeare authorship question and Edward de Vere's part in it.

At the time Looney introduced the idea of de Vere's authorship, literary criticism consisted of two complementary approaches to the study of literature. One approach sought to explain the significance of works of literature by considering them as works of art important in themselves. Practitioners of this approach, who we could call literary connoisseurs, sought to understand and demonstrate the technical perfection or artistic unity of a work. They helped readers understand the genre, literary devices and rhetorical figures used, and express a judgment about how successfully the author used them.

The other approach sought appreciation of works of literature through knowledge of the life and times of their authors. We might call practitioners of this approach literary historians. Their work is of greater relevance for the Shakespeare authorship question because they seek to understand the author's intentions and how he or she was influenced by the political, economic, social and literary currents of his or her society. Because authors lived and worked in times different from our own, the general reader can benefit from the expert knowledge of the author's life and times that literary scholars bring to the discussion.

Given what was to come, it is important to emphasize that the two approaches are two sides of one methodological coin, the coin being the humanist tradition of literary criticism. In that tradition, professor Jonathan Culler explains,

the task was the interpretation of literary works as the achievements of their authors, and the main justification for studying literature was the special value of great works: their complexity, their beauty, their insight, their universality, and their potential benefits to the reader.<sup>52</sup>

The two approaches were in rough balance in the middle of the twentieth century – a balance that was not to last long because the humanistic tradition itself began to lose favor around this time with the academic and scholarly world. By the last few decades of the twentieth century, that tradition – one not unfavorable in itself to consideration of the Shakespeare authorship question – was largely replaced by a new methodology that does not value close readings of literary works and in which the intentions of the author are largely irrelevant.

One of the first developments in the transformation of literary criticism came from within the historical approach – the change in emphasis from seeking to understand those aspects of an author's society that he consciously and purposely sought to portray in his works to what he unconsciously revealed about it. It is a change in focus from what Lionel Trilling identifies in *The Liberal Imagination* as “the explicit statements that a people makes through its art . . .”<sup>53</sup> to that of “a culture's hum and buzz of implication . . . the whole evanescent context in which its explicit statements are made. It is that part of a culture which is made up of half-uttered or unuttered or unutterable expressions of value.”<sup>54</sup>

Literary scholars can bring expert knowledge to help readers “to reconstruct the original context of production (the circumstances and intentions of the author and the meanings a text might have had for its original readers),” and to “expose the unexamined assumptions on which a text may rely (political, sexual, philosophical, linguistic).”<sup>55</sup>

In seeking to “expose the unexamined assumptions” of an author, we have reached what W. K. Wimsatt and Monroe Beardsley called “the intentional fallacy,” in which “the design or intention of the author is neither available nor desirable as a standard for judging the success of a work of literary art.”<sup>56</sup> “The meaning of a work is not what the writer had in mind at some moment during composition of the work, or what the writer thinks the work means after it is finished,” Culler explains, “but rather what he or she succeeded in embodying in the work.”<sup>57</sup> Thus, a fuller examination of works of literature is required, one that examines both the conscious and unconscious results of the author’s efforts.

Another development came from within the artistic approach to the study of literature. After the heyday of The New Criticism, some critics adopted its practice of separating works of literature from their authors, but did so not in order to examine them as works of art as the New Critics did, but to examine their political and social content unencumbered by the intentions of the author – exactly those aspects of the work that The New Critics had sought to get away from by isolating works from their authors and history.

With both approaches focused on the contents of the work of literature rather than the author, there was, some thought, no need to consider the author at all. Why not eliminate consideration of him or her completely in order to focus directly on the contents without distinguishing between its intentional or unintentional origin? With this line of thinking we have reached what Roland Barthes called “the death of the author.”

The approach of examining works of literature in isolation from consideration of their authors is obviously not one favorable to the Shakespeare authorship question. We have already seen attempts to cut off consideration of the strongest type of support for the idea of de Vere’s authorship – the correspondences between his life and Shakespeare’s works – by denying the validity of circumstantial evidence. We now see another tactic that would have the same effect: that of denying the importance of the author and thus the importance of any linkages between de Vere’s life and Shakespeare’s works.

Oxfordians have speculated among themselves for years about the extent to which the “death of the author” approach to literary theory arose as a response to the mismatch between Shakespeare’s life and Shakespeare’s works. It is perhaps not unreasonable to consider the extent to which literary scholars, convinced that the man

from Stratford was Shakespeare the writer, deliberately overstated “the death of the author” as a way of preserving their belief in his authorship.

There is yet one more significant change to consider: the change from studying works of literature through the history of their times, to studying societies and cultures through works of literature. In this methodology, literary criticism is no longer an independent field of study, but one that has been largely subsumed as a subfield within the larger subject of Cultural Studies.

Rather than being the ends to be studied, literary works have become merely one means through which non-literary subjects are studied. Cultural theorists regard literary works of all types as mere cultural artifacts to be mined for data about the society from which they arose in the same manner that advertising or other anonymously-written documents are examined. Considering works of literature as works of art important in themselves – the work of literary connoisseurs – has little place in this methodology, and has largely ended within academia. Gone is any sense that literature has something meaningful to say about the larger aspects of what it means to live as human beings on planet earth. The focus is now on what specific works can tell cultural researchers about specific political, economic, social or sexual practices in the culture from which they arose.

Let us be clear that when the so-called “death of the author” is discussed, what is also implied is the death of literary criticism itself. The standard anthology in the field, *The Norton Anthology of Theory and Criticism*, declares that

Literary texts, like other artworks, are neither more nor less important than any other cultural artifact or practice. Keeping the emphasis on how cultural meanings are produced, circulated and consumed, the investigator will focus on art or literature insofar as such works connect with broader social factors, not because they possess some intrinsic interest or special aesthetic value.<sup>58</sup>

The introduction to another widely used text, *Cultural Studies*, specifies that “although there is no prohibition against close textual readings in cultural studies, they are also not required.”<sup>59</sup> Literature, Jonathan Culler explains, can be mined for information about cultural issues unrelated to any consideration of the intentions of the author.

Interpreting *Hamlet* is, among other things, a matter of deciding whether it should be read as talking about, say, the problems of Danish princes, or the dilemmas of men of the Renaissance experiencing changes in the conception of the self, or relations between men and their mothers in general, or the question of how representations (including literary ones) affect the problem of making sense of our experiences.<sup>60</sup>

In all of these potential “interpretations,” the play is treated as just another cultural artifact, in which what is most special about it – that it was created by a specific human being for a specific purpose or purposes – is intentionally ignored.

The Department of Literature still exists on university campuses today, but often it functions as a Department of Cultural Studies. As professor James Seaton observes, “in some of the most influential academic centers literary criticism has been replaced by cultural studies.”<sup>61</sup> The situation is not that cultural studies courses are taught alongside literature courses in those departments. It is not even that cultural studies have influenced the methodology of literary criticism to include new factors in literary criticism. It is, rather, that a take-over has occurred in which there appears to be little room left for the traditional humanistic approach to literary studies. Seaton notes that “From the viewpoint . . . of influential English graduate programs, prestigious academic journals, authoritative anthologies of criticism, and the most prominent academic theorists, the humanistic tradition in literary criticism seems to be invisible.”<sup>62</sup>

As one example, the editors of *The Norton Anthology of Theory and Criticism* could not find much space in their 2,785-page volume for the giants of traditional humanistic literary criticism in the twentieth century. Lionel Trilling, for instance, is not represented at all, and Edmund Wilson is represented only by one unrepresentative essay, even though the book claims to “present a staggeringly varied collection of the most influential critical statements from the classical era to the present day.”<sup>63</sup>

To sum up, the humanistic tradition of the study of literature has been replaced by one unreceptive to the authorship question. The methodology of seeking correspondences between events and characters in literary works and events and people in the life of a purported author has little resonance in an environment in which the author is regarded as an outmoded “construct” that is bypassed in favor of cultural forces which determine the content of literary works. Simply put, the authorship question is not one that most literary scholars find attractive in the current environment.

## Methodology and the Future of Oxfordian Theory

When I began drafting this paper I had expected to find that the incompleteness of the two theories was the principal reason they had been rejected in the 1920s, and that the completion of the scientific theory and the continuing non-completion of the literary one explained the difference in their fate. However, as disconcerting as the incompleteness of Oxfordian theory may be for many Oxfordians, I have concluded that it is only a contributing factor to the theory’s failure to gain traction within academia.

It now appears the most important factor affecting acceptance or rejection of new

theories is that of methodology, that of the process through which scientific or academic communities pursue new knowledge and interpret and judge new ideas and data. Several conclusions about the critical nature of methodology can be drawn from this paper that account for academia's continuing rejection of the Shakespeare authorship question.

First, methodologies differ from field to field and must be suited to the nature of the objects being examined and the explanations being sought. For historical studies, the appropriate methodology is the "adductive reasoning" explained by historian David H. Fischer that asks open-ended questions and answers them in the form of reasoned argument. For literary criticism, the appropriate methodology is one that recognizes the two distinctive features of works of literature: that they are unique and so deserve careful study in themselves as works of art, and they are produced by specific individuals for specific reasons at specific points in time, so awareness of the author's intentions and the details of his life and times will increase our understanding of them. The Shakespeare authorship question, being a study of the historical aspects of the origin of works of literature, will best be studied through a methodology blending history and literary criticism.

Second, focus must remain on substantive accomplishments, not on adherence to any given methodology. Facts, data and theories must be considered separately from the methodology in place when they were discovered. Not doing so was the mistake made by American geologists when they rejected Wegener's theory, and it is a mistake being made by literary scholars who reject findings by Oxfordians today.

Third, the right type of data must be selected and it must be judged objectively. Data cannot be invented, but must be found. Inventing new data in the absence of facts was the flaw in the creation of the ideas of sunken continents, land bridges, and the play *ur-Hamlet*. Ad hoc explanations are not legitimate explanations.

And fourth, circumstantial evidence is a legitimate form of evidence in historical investigations, just as it is in courtrooms. Correspondences between events and characters in literary works ascribed to a pen name and similar events and people in the life of a purported author are legitimate grounds for establishing authorship.

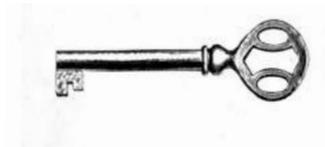
If the study of literature is to occur under a new methodology, it must take place outside the dominion of and domination by Cultural Studies. Because the two fields study different subjects they require different methodologies, and thus need to be housed in different departments dedicated to maintaining high standards in their respective methodological areas.

In the effort to separate literary studies from cultural studies, it could be the case that the authorship question will be the issue that triggers changes in the broader methodology of literary criticism. The difficulty of the effort to reconcile the life of the

man from Stratford and the works of Shakespeare could be the catalyst leading to the return of genuine literature programs in our universities.

Once truly independent literary studies departments are established or re-established, safe havens will exist for the methodology of literary studies. In them, literary scholars will be free to cultivate what one historian describes as “the ability to enter imaginatively into the life of a society remote in time or place, and produce a plausible explanation of why its inhabitants thought and behaved as they did.”<sup>64</sup> Applying this ability to the study of literature, they will seek to step outside their own personal experiences, to see the world as the author saw it in another time and place and to understand what he or she had to say about it.

A methodology of literary criticism that is able “to make the great works of literature more consequentially available not only to academics but to general readers without any special intellectual equipment beyond the educated good sense of their time,”<sup>65</sup> as James Seaton phrased it, is one in which the study of the Shakespeare authorship question would finally receive a fair hearing.



## Works Cited

### Continental Drift

- Christian, David. *Maps of Time: An Introduction to Big History*. Berkeley: University of California Press, 2013.
- Du Toit, Alexander L. *Our Wandering Continents: An Hypothesis of Continental Drifting*. Edinburgh and London: Oliver and Boyd, 1937.
- Oreskes, Naomi. *The Rejection of Continental Drift: Theory and Method in American Earth Sciences*. New York: Oxford University Press, 1999.
- Popper, Karl. *Conjectures and Refutations*. New York: Basic Books, 1962.
- Wegener, Alfred. *The Origin of Continents and Oceans*. Translated by John Biram. New York: Dover Publications, Inc., 1966.

### Literary Criticism

- Culler, Jonathan. *Literary Theory: A Very Short Introduction*. New York: Oxford University Press, 2009.
- Elliott, H. J. *History in the Making*. New Haven: Yale University Press, 2012.
- Fischer, David Hackett. *Historians' Fallacies: Toward a Logic of Historical Thought*. New York: HarperCollins, 1970.
- Fish, Stanley. *Save the World On Your Own Time*. New York: Oxford University Press, 2008.
- Seaton, James. *Literary Criticism from Plato to Postmodernism: the humanistic alternative*. New York: Cambridge University Press, 2014.
- Trilling, Lionel. *The Liberal Imagination: Essays on Literature and Society*. New York: New York Review Books, 2008. [reprint of the 1950 edition]
- Wimsatt, William K. Jr. and Monroe C. Beardsley. "Intentional Fallacy," [1946, revised in 1954], from <[letras.cabaladada.org/letras/intentional\\_fallacy.pdf](http://letras.cabaladada.org/letras/intentional_fallacy.pdf)> March 2015.

### The Shakespeare Authorship Question

- Looney, J. Thomas. *"Shakespeare" Identified*. London: Cecil Palmer, 1920.
- "The Earl of Oxford as 'Shakespeare: New Evidence.'" *The Golden Hind*, Vol.

1, No. 1, October, 1922, p. 23-30.

Ogburn, Charlton. *The Mysterious William Shakespeare*. McLean, VA: EPM Publications, Inc., 1992.

Ward, Colonel B. R. "The Shining Possibilities of Anne Vavasour." *The Shakespeare Pictorial*, September, 1931, p. 148.

Wright, Louis Booker. "The Anti-Shakespeare Industry and the Growth of Cults." *The Virginia Quarterly Review*, Vol. 35, No. 2 (Spring 1959). Reprinted in Alexander Burnham (editor). *We Write for Our Own Time: Selected Essays from 75 Years of the Virginia Quarterly Review*, pp. 105-115. Charlottesville: University Press of Virginia. 2000.

## Notes

- 1 Naomi Oreskes, *The Rejection of Continental Drift*, p. 5.
- 2 Oreskes, *Rejection*, p. 62.
- 3 J. Thomas Looney, "*Shakespeare*" *Identified*, p. 151.
- 4 Looney, *Identified*, p. 493.
- 5 Looney, *The Golden Hind*, Vol. 1/1, p. 30.
- 6 Oreskes, *Rejection*, p. 57. Internal quote is from Alfred Wegener, *The Origin of Continents and Oceans*, p. 76.
- 7 Looney, *Identified*, p. 80.
- 8 Looney, *Identified*, p. 174.
- 9 Looney, *Identified*, p. 175.
- 10 Charlton Ogburn, *The Mysterious William Shakespeare*, p. 198.

- 11 Oreskes, Rejection, p.125.
- 12 B. R. Ward, *The Shakespeare Pictorial*, p. 148.
- 13 Oreskes, Rejection, p. 6.
- 14 Oreskes, Rejection, p. 313.
- 15 Oreskes, Rejection, p. 313, 316.
- 16 Oreskes, Rejection, p. 6.
- 17 Oreskes, Rejection, p. 314.
- 18 Oreskes, Rejection, 126.
- 19 Oreskes, Rejection, p. 145.
- 20 Oreskes, Rejection, p. 139.
- 21 Oreskes, Rejection, p. 124-126.
- 22 Oreskes, Rejection, p. 153.
- 23 Oreskes, Rejection, p. 126.
- 24 Oreskes, Rejection, p. 154.
- 25 Oreskes, Rejection, p. 154.
- 26 Looney, *The Golden Hind*, p. 23, 24.
- 27 David Hackett Fischer, *Historians' Fallacies*, p. xx and 4.
- 28 Oreskes, Rejection, p.56-57.
- 29 Oreskes, Rejection, p. 218.
- 30 Oreskes, Rejection, p. 295.
- 31 Oreskes, Rejection, p. 295.

- 32 Oreskes, Rejection, p. 218.
- 33 Oreskes, Rejection, p. 218.
- 34 David Christian, *Maps of Time*, p. 69.
- 35 Oreskes, Rejection, p. 218.
- 36 Oreskes, Rejection, p. 6.
- 37 Oreskes, Rejection, p. 313.
- 38 Oreskes, Rejection, p. 156.
- 39 Louis Booker Wright, “The Anti-Shakespeare Industry and the Growth of Cults,” quoted in Charlton Ogburn, *The Mysterious William Shakespeare*, p. 154.
- 40 Alexander Burnham (editor), *We Write for Our Own Time: Selected Essays from 75 Years of The Virginia Quarterly Review*. Charlottesville: University Press of Virginia, 2000, pp. 105-115.
- 41 Christian, Maps, p. 71.
- 42 Christian, Maps, p. 70.
- 43 Christian, Maps, p. 71.
- 44 See, for instance, Hank Whittemore’s *The Monument* (2005) and Charles Beaulier’s *Shakespeare’s Lost Kingdom* (2010).
- 45 Oreskes, Rejection, p. 275.
- 46 Oreskes, Rejection, p. 290.
- 47 Oreskes, Rejection, p. 279.
- 48 Oreskes, Rejection, p. 289.
- 49 Popper explains this method most completely in *Conjectures and Refutations*. (New York: Basic Books, 1962, pp. vii, 37, 46, 55 and 69-70.)

- 50 Oreskes, Rejection, p. 260.
- 51 Oreskes, Rejection, p. 260-261.
- 52 Jonathan Culler, *Literary Theory*, p. 47.
- 53 Lionel Trilling, *The Liberal Imagination*, p. 205.
- 54 Trilling, *Imagination*, p. 206-207
- 55 Culler, *Literary Theory*, p. 68-69.
- 56 William K. Wimsatt, Jr., and Monroe C. Beardsley, "Intentional Fallacy," p. 1.
- 57 Culler, *Literary Theory*, p. 67.
- 58 Vincent B. Letich (General Editor), *The Norton Anthology of Theory and Criticism*, 2<sup>nd</sup> edition. New York: Norton, 2010, p. 2478. Quoted in James Seaton, *Literary Criticism from Plato to Postmodernism*, p. 20.
- 59 Lawrence Grossberg, Cary Nelson, and Paula Treichler (eds.) *Cultural Studies* (New York: Routledge, 1992, 2. 4. Quoted in Jonathan Culler, *Literary Theory*, p. 50.
- 60 Culler, *Literary Theory*, p. 33.
- 61 James Seaton, *Literary Criticism from Plato to Postmodernism*, p. 1.
- 62 Seaton, *Literary Criticism*, p. 6-7.
- 63 Letich (General Editor), *The Norton Anthology of Theory and Criticism*, 2<sup>nd</sup> edition. New York: Norton, 2010, p. 2478. Quoted in Seaton, *Literary Criticism from Plato to Postmodernism*, p. 20.
- 64 H. J. Elliott, *History in the Making*, p. xi.
- 65 Seaton, *Literary Criticism*, p. 10.