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On Explaining the Trial of Galileo

Organon 8, 137-166

1971

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ON EXPLAINING THE TRIAL OF GALILEO [1]

> If a science has to be supported by fraudulent means, let it perish.
>
> (J. Kepler)

> The life of a great man... can never be a mere record of undisputed fact... The biographer... must penetrate behind mere events to the purpose and character they disclose, and can only do so by an effort of constructive imagination.
>
> (A. E. Taylor)

> There are no villains in the piece... It is what men do at their best, with good intentions, that really concerns us... if Joan had not been burnt by normally innocent people in the energy of their righteousness, her death at their hands would have no more significance than the Tokyo earthquake...
>
> (G. B. Shaw)

1. INTRODUCTION AND SUMMARY

Arthur Koestler's *The Sleepwalkers* is, the subtitle says, "a history of man's changing vision of the Universe". The thesis which this history illustrates, we are told, is that science and religion are essentially partners, sharing mystic intuition as their common source. Admittedly, science and religion are now divided, but this division is merely the outcome of some historical events—Galileo's quarrel with the Church

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[1] I am indebted to Professors J. Clark, S. J., F. Cronin, S. J., I. C. Jarvie, Sir Karl Popper, and G. de Santillana. None of them endorses all the views here proposed. References are given only when it is not clear from the context to which work mentioned in the bibliography I am referring; numbers in brackets are page numbers of works named in the bibliography.

of Rome and the ensuing misunderstandings—which could have been avoided; it is high time now to reunite science and faith, so as to save Man from (nuclear) selfdestruction. The meat of the volume consists of two essays, one on Kepler, one on Galileo. The former is sympathetic towards its hero and was well-received, the latter is critical of its hero and was frowned upon. *Prima facie*, at least, public opinion was rather apologetic.

Koestler's view of Galileo has been violently attacked by two leading students of Galileo—Santillana and Drake. Many of their strictures are just; yet even if all of them were just, I would still dissent from their overall judgment. Though Koestler is no scholar, his work is of value. It contains valid criticisms of scholary works, and the first lively image of Galileo. Koestler's great success is in managing the humanization of an idol even though he did so at the expense of committing some serious errors.

Roman Catholic writers have already claimed that the clash between Galileo and the Church was to this or that extent rooted in three faults of Galileo's. First, his difficult personality; second, his mistakes in science which were corrected by later scientific studies; and, third, his attempts to meddle with theology. Koestler, although his contempt for the Inquisition is considerable, accepts these strictures, pushes all three as far as he can, and combines them into one: Galileo fought not for the sake of truth, but because he was pathologically unable to avoid any quarrel, accept any compromise, or admit any error. Koestler has succeeded in drawing a new and very vivid picture of Galileo and of his trial, even though hardly any of the accusations he levels against Galileo is claimed to be new, with the exception of the attribution of motives. Now it is dangerous to attribute motives, especially low ones, and especially to an idol. Yet Koestler has rendered a valuable service in trying to do so, and not merely because he may be viewed as the devil's advocate. We cannot explain historical events without making hypotheses concerning the aims, interests, and motives of those who have participated in them. And after we propose such explanatory hypotheses, we can try to argue rationally about their truth or falsity, and then improve on them. Galileo held three different positions in different times: first, he concealed his Copernicanism; then—when he was about fifty—he defended it with some caution; his great battle for it took place when he was about seventy. Koestler's view of Galileo's motives and purpose is offered as one explanation of all theree phases: the young Galileo's avoidance of open defence of Copernicanism was rooted in his knowledge that once he would be drawn into a controversy it would be a fierce and uncompromising battle. This self-awareness, says Koestler, first made Galileo timid; but once he fought he would not be stopped. For my part, I think the story of Galileo is not that of one and

the same character: the story of the old Galileo is the story of a changed man—of a Catholic reformer who hoped to prevent further clashes between science and Catholicism, but whose plan misfired.

2. KOESTLER AND HIS PREDECESSORS

The story of Galileo has two sides to it, one scientific, one political, which are closely related but very distinct. It has other sides as well, philosophical, theological, *etc.* These have not yet attracted the attention they deserve. Let us, then, take the scientific side first and the political second. Most writers on Galileo have taken it for granted that scientifically he was right because he was a Copernican, and his opponents were wrong as they wery defending Aristotelianism. Some Roman Catholic writers, however, have accused Galileo while defending, or at least refraining from discussing, the scientific position of the Church.

A. C. Crombie's position may serve as an instance: it is apologetic, eclectic, and up-to-date. If I understand Crombie rightly, he produces three arguments against Galileo. First, Galileo was mistaken in having claimed that Copernicanism had been demonstrated, since the demonstration of Copernicanism was produced only in the early nineteenth century, with the discovery of stellar parallax [323]. Secondly, as Einstein has shown, the truth is that there is no immobile centre of the world; the physicist's choice of a centre is arbitrary [327–8]. Thirdly, science concerns itself not with the search for truth but with the proposal of working hypotheses which save the phenomena [324–5, 328]. (Firstly, I did not borrow the bowl from you; secondly, I already returned it to you; and thirdly, it was cracked when I got it.) These arguments have little reference to the historical situation; even were they correct they should be dismissed as mere hindsights.

Koestler's attack on Galileo the scientist includes Crombie's main ammunition, and, in addition, the view that Galileo was lying when he claimed that Copernicanism had been demonstrated. This view I consider to be false, but not outrageous, and at least historical rather than anachronistic. Those who are infuriated by it are, I suppose, adherents of the principle that men of science are invariably honorable men, and *vice versa*. This principle is very widely accepted, as I have tried to show elsewhere. Even Koestler, who explicitly rejects it, abides by it throughout his work, to the point of acknowledging Galileo as a great scientist only after the Inquisition broke his pride in 1633. This terrible opinion obviously clashes with ample evidence. Since Koestler usually endorses Kepler's judgment as sane, moderate, and human, I shall metion only Kepler's view of Galileo. In his comments on Galileo's *Assayer* of 1623, Kepler makes it quite obvious that in his view Galileo

was a powerful thinker [355], but far from being an easy and pleasant character. Kepler himself claims there [341–2] that Galileo made statements he did not intend to make, merely in the heat of the debate. Of course, Koestler's view that Galileo's statements to the Inquisition were lies goes much further; and, unlike Kepler's is nourished not by scientific considerations but by the Catholic hostile literature. But this is no reason to dismiss Koestler's view as false, though ultimately it has to be rejected—for other and better reasons.

Koestler's attack on Galileo the scientist is nourished not only by the hostile Catholic attitude, but also by the clumsily apologetic attitude of others. To take one example, in his introduction to his selection from Galileo's *Assayer* [226] Drake speaks of him as the teacher of experimental philosophy (a phrase which Galileo never used). On the next page, he brushes aside the fact that Galileo's *Assayer* concerns the defence of a false hypothesis, according to which comets consist of (earthly) vapors, adopted in order to rescue his own version of Copernicanism. Drake claims that this is quite irrelevant to "the main point of the book, which lies not in the hypothesis itself but in its use". Santillana comes to Galileo's rescue in another way [153]. It was right, as I understand Santillana, to dismiss the comets until it was possible to use them as demonstrations of Copernicanism; and this only Newton could do. Obviously, this is both apologetic and untrue. Koestler's condemnation of Galileo the scientist on the ground that he did not plot the elliptic courses of comets is faintly amusing, until one compares it with the clumsy apologies that Galileo's defenders put forward to explain his failure to endorse the ellipse. Generally, every time Galileo's defenders are apologetic, Koestler stresses the point which causes them discomfort; and every time the Catholic apologists show a weak point in Galileo's scientific views Koestler follows them. And he regularly attributes some unpleasant motives to Galileo. But let us leave the scientific side of the story for now.

The chief Catholic technique used on the political side of the story is that of pooh-poohing one decument and stressing another; and Koestler follows suit. Let us see how the political evidence is handled by James Brodrick, the biographer of Cardinal Bellarmine (1928). Santillana, no ally to him, refers to him as to "no less a historian than Father J. Brodick, S. J.", attributing to him "discriminating scholarship" and "Christian warmth". Brodrick puts the whole blame for the Church's denunciation of Copernicanism in 1616 on Galileo. This thesis, looking rather shaky, must needs be heavily supported by evidence. Brodrick quotes—from a letter written by the then (1616) Tuscan ambassador to Rome—some unpleasant words about Galileo's behavior in Rome and about the imprudence of a (young) Cardinal whom Galileo presumably had sent to talk to the Pope about Copernicanism. Copernicanism was denounced, Brodrick tells

us, as the result of Galileo's pressure, which the Tuscan ambassador was describing. Brodrick adds a footnote protesting against Galileo's defenders (of before 1928): "insinuating that the ambassador was a bit of a fool", and ignoring the alleged fact that he was a personal friend of Galileo, they dismiss or ignore his evidence.

On the same page where he launches this complaint, Brodrick himself suppresses a part of the same letter, which contradicts his own story. According to Brodrick's story, once Galileo extended enough pressure to get the machine going, it had to go on its own course according to established rules of procedure: the Congregation of the Holy Office had to consult experts about the status of Copernicanism, and to endorse the experts' judgment. Yet according to the same letter of the Tuscan ambassador, the question was decided not at all by the Congregation, not by any consultation, and according to no rules of procedure: the obscurantist Pope and Cardinal Bellarmine were determined to condemn Copernicanism by hook or by crook.

It is possible to defend Brodrick's omission: the ambassador's story may be questioned because of being chronologically inconsistent with the Vatican files. But this defence of Brodrick will show how much he distorts the views of Galileo's defenders when he says that they had dismissed the ambassador's evidence for no good reason. According to the Tuscan ambassador the decision against Copernicanism was taken by the Pope and Bellarmine on March 2nd 1616, whereas according to the Vatican records the Congregation had passed its verdict against Copernicanism on February 25th. The ambassador says that the Pope has told the young Cardinal who supported Galileo that "the question was to be referred to the Cardinals of the Holy Office", sometime after the question had already been referred to these Cardinals, and decided upon by them. The Pope could not have suggested on March the 2nd to the young Cardinal "to persuade him [Galileo] to give up that opinion [Copernicanism]" if on February 26th Bellarmine had already forced Galileo to be silent.

Santillana seems to explain away the ambassador's derogatory remarks on Galileo in two ways. First, he views the ambassador as "a cynical man of the world" who had deserted his acquaintance Galileo. Secondly, he thinks that the misinformation in his letter is a sign of his having been deceived by a prearranged leak designed to put the blame on Galileo. Santillana has not made up his mind as to whether the ambassador was a mere acquaintance and "a cynical man of the world", or a naive friend who was taken in by misleading inside information which he could easily have refuted by hearing from Galileo about Bellarmine's threats. (All *dramatis personae* were present in Rome then.)

As Santillana's defence is so weak, Koestler has little difficulty here;

he could, and did, follow Brodrick's position rather closely, and, in addition, stress every piece of evidence concerning which the defenders of Galileo have tried to gloss over somewhat more glibly then they ought to have done. This is precisely the reason for my viewing his work as much more significant than that of other critics of Galileo, like Crombie or Brodrick. Even if we reject all of his views, and I doubt that we can do this, we must admit that he has posed quite a number of serious problems for those of us who side more with Galileo than with Bellarmine. In any case, it is to be hoped that we shall never be able to return to the old idealized picture of Galileo.

3. KOESTLER'S PEN-PORTRAIT OF GALILEO AND ITS IMPLICATIONS

Koestler describes Galileo as an extremely unplesant and arrogant person, whose interest in science was much less a motive for his actions than his "hypersensitivity to criticism, and his irrepressible urge to get involved in a controversy" [432, 470]. Although great men of science may be as negative personalities as Koestler's Galileo, and although one cannot entirely refute Koestler's picture of the young Galileo (it can easily be shown to contain exaggerations), one can show that the older Galileo found his personal salvation in science: he learned to rise to the occasion of defending the cause of science, and thus vastly improved his personal character. (It seems that Koestler's historical figures suffer from the same defect as his fictional figures: they never change their characters.)

According to the accepted view of the trial, the Pope and his advisers used the trial for an attack on science in general; it was a piece of self-assertion by obscurantists. Catholic historians, too, tend to accept this view. But this view conflicts with the following well-known facts. The new Pope, Urban VIII, had encouraged Galileo to write the *Dialogue* or at least let Galileo come out of six audiences with him with the impression that such was the case (which is practically the same thing). And the book won the *imprimatur*. Koestler argues that the encouragement was given to the scientist Galileo and that the trial was of the conceited and quarrelsome Galileo who took the opportunity and fought his own private battle instead of fighting for the cause of science; that by bullying ignorant censors Galileo succeeded in getting the *imprimatur* for a book of a different character than the one he was expected to write; that his bluff was called in a very short time, and he had to pay for his arrogance.

Assuming that the existence of the Inquisition and censorship are not exactly encouragements to the freedom of thought, and assuming that the Inquisition was not created for the sole purpose of intimidating

arrogant scientists, Koestler's contention that Galileo's character was the cause of the trial cannot be taken literally. It must be reinterpreted as an exaggeration of the following view. Unfavorable for science as the social and political situation was, it was not so bad to make it impossible for a man of science to defend Copernicanism reasonably and get away with it. It might have been possible—and, for the sake of science, highly advisable—to get away with a defence of Copernicanism by making a verbal concession so as to allow opponents to save face. Given the atrocious illiberalism of the times, the Church's behaviour was reasonable in view of Galileo's violent outbursts, extravangant claims, stubborn unwillingness to compromise, and immense capacity to annoy and irritate practically anybody. Thus, private affairs upset public matters and led to the divorce between Faith and Science.

It is this reinterpretation of Koestler's presentation which will be examined here. We shall take the hypothesis, examine what it can explain, what facts may conflict with it, and how it can be replaced by a better one. The hypothesis is, that Galileo was clever, vain, and hypersensitive. It follows, first, that he quarrelled obsessively, and second, that knowing his own weakness, he cleverly avoided quarrel as much as his vanity permitted. It follows, further, that he was a rather unpleasant person; not as friendless as Koestler makes him to be, but not too popular either. The historical facts the hypothesis explains are rather numerous. The hypothesis explains all the unpleasant details of Galileo's early career; in particular why, until 1613, when he was almost fifty years old, Galileo never committed himself publicly to Copernicanism: as we concluded from the hypothesis, he tried to avoid controversy whenever his vanity permitted. After he had declared his allegiance to Copernicanism, in 1613, he had, as we have concluded, to quarrel obsessively, even if the result was that he destroyed himself. The problem remains: why did he start defending Copernicanism? The answer must be, his vanity prevented him from concealing his views any longer. How? Having made some astronomical discoveries, Galileo could not but stress, not to say exaggerate, their significance (because of his vanity). In other words, he could not avoid the temptation of viewing his discoveries as demonstrations of the truth of Copernicanism.

This part of Koestler's story is logically neat. The evidence which he marshals in its favor is hardly new, but he has shown how glibly this evidence has been glossed over; yet he exaggerates its value, and makes it go a long way. Almost all his significant evidence relates to the period up to 1613. Up till then Galileo picked only quarrels which he could win and which did not endanger his career. His commitments to Copernicanism were vague enough not to cause him any serious trouble; he had a quarrel with some minor Dominicans about it, but he

was confident that he could win; and he did win. But there is evidence (see below) which contradicts the hypothesis: it is a fact that in 1613 he was psychologically quite capable of ignoring a challenge. Thus Koestler's story up to 1612, though highly exaggerated in its psychology, and very incorrect in disregarding Galileo's early and passionate interest in truth, is nonetheless largely correct in describing him as an unpleasant, career-seeking, and rather touchy, quarrelsome fellow. In 1614 we find Galileo engaged in a battle with the mighty Bellarmine, in which he does endanger his career and even his life. Since Galileo was not as hypersensitive as Koestler makes him out to have been, he must have had some reason for suddenly becoming so reckless. Let us, then, consider the following midification. Galileo had been career-minded until about the age of fifty. But then he achieved great fame, and learned that truth was much more important to him than fame. Many people seek fame while neglecting to fight for the truth, deceiving themselves all the time that their motive is respectable: once they have achieved fame, they say, they will be able and willing to fight effectively for the cause of truth. It is quite possible that Galileo held the same attitude and carried out his plan sincerely. And when he started his fight he soon found out that fame is of no importance one way or another. Career-seekers normally deceive themselves in thinking that when they have achieved position and fame they will use it in their fight for the cause of truth. Yet occassionally they are sincere. Assuming that Galileo was one of them, one has to admit that he was also sincere. It must always be remembered that he was an unusual person; possibly he looked all the time for a chance of getting his ideas across without a battle; in any case, we know that he fought, and rose to the occasion wonderfully.

4. THE FALL OF GALILEO

Galileo's visit to Rome in 1611, after publishing his *Starry Message* (1610), was a great success. It was a very dramatic change from his previous status. Just before his journey to Rome he had been criticized rather sharply, and his astronomical discoveries had been declared chimerical. Only Kepler had defended them, and upon faith, not after examination of the evidence. Kepler had been worried by the fact that all the evidence he had heard was opposed to Galileo's evidence, and had asked Galileo to name witnesses confirming his evidence. Galileo could not name any witness, very much to his frustration and chagrin; and only with effort—so his letter to Kepler reads—could he turn his immense bitterness against his colleagues into scorn at the multitude, their stupidity, and their ignorance.

And then, in Rome, he was received like a king. The Jesuit astrono-

mers of the Roman College endorsed most of his observations, and he found quite a few friends among them. He was cordially received even by Cardinal Bellarmine, who had been Professor of Philosophy, Master of Controversies, Rector of the Roman College, and remained one of the most powerful figures in Rome until his death. Bellarmine's recognition of Galileo was no small matter, especially considering that Bellarmine was a staunch authoritarian and traditionalist, and had been one of the judges of Giordano Bruno, who had been burnt on the stake but one decade before, whereas Galileo was a follower of Bruno (see below), or at least suspected of being the follower of Bruno, as even Koestler cryptically admits. (Koestler's dismissal of the thesis that Bruno was a martyr of science is unworthy of criticism.)

Of course Galileo found opposition, too, which is not surprising. But in his case one might have expected all opposition to have become weaker and weaker with the passage of time. Yet Galileo's position worsened—and very quickly too. In 1611 Bellarmine shows his esteem of Galileo; in the middle of 1612 Bellarmine writes to Galileo, expressing his affection, respect, and readiness to be of any service (the function of this most unusual letter has not been studied as yet). And then comes a change: in 1613 Bellarmine speaks against Galileo in a private conversation with a priest who is a friend of Galileo, quoting Psalm 19 which describes the motion of the sun; the point is discussed in Galileo's *Letter to Castelli* of December 1613, and in at least one of Bellarmine's sermons [Brodrick, 335], preached soon afterwards. (Like most of Galileo's early works, this one was unpublished and privately circulated; Bellarmine's sermon was published early in 1615, but, very likely, it was also privately circulated in 1614.) All is still within the bounds of civility; the rest of the story is not. In April 1615 Bellarmine creates a new nuance by launching a warnings, suggesting that Galileo's opinion is opposed to a decision of the Council of Trent (this is no small threat), and contradicts King Solomon (*Ecclesiastes*, "and the earth for ever standeth"), the wisest of all men; in about November or early December 1615 he expresses displeasure at Galileo's plan to come to Rome. This is reported by the Tuscan ambassador on December 5th, 1615, but Galileo is in such a hurry to leave Florence that he never receives the ambassador's warning—he is in Rome on the 7th of December. (Koestler's report is inaccurate; almost every one of his inaccuracies can be traced back to a lack of clarity of Santillana or Drake.) The situation is so baffling that until Gebler's work of a century ago it was assumed that Galileo was summoned to Rome. As Gebler shows [71f], he came to Rome voluntarily. Gebler explains this disastrous move as the result of Galileo's unawareness of the strength of his opponents' dogmatism. This explanation is a bit naive, yet Santillana accepts it as he has no better one to offer.

Two months after Galileo's arrival in Rome nothing seems to have happened; then the pace guickens considerably. On February 18th, 1616 (or a little earlier), Galileo goes to the Holy Office [Santillana, 114–5]; on that day the Congregation assembles and on the next day asks for expert opinion; though the problem is most difficult, the expert opinion against Copernicanism is procured within a week; the Congregation is assembled again on the 25th; on the same day the decision is reached *and* ratified *and* Bellarmine is instructed to act; he acts on the 26th, summoning Galileo to his palace and telling him to be silent or else.

This chronology is based chiefly on the Vatican files. It is contradicted, we remember, by the Tuscan ambassador's letter of March 4th. Far be it from me to prefer the (possibly doctored) Vatican files to the ambassador's (second hand) information or *vice versa*. It is possible that the ambassador's letter had been written two weeks before it was dated, to be first delayed and then sent in a hurry; and other explanations are possible. But, obviously, there was a great rush and tumult. Why? Why?

The decision condemning Copernicanism, which was taken on February 25th, 1616, was not published until years later, and perhaps it was not intended for publication at all. It was acted upon, at least according to the Vatican files. The Vatican files report the decision of February 25th and Bellarmine's warning to Galileo on February 26th. The public, however, knew only of a decree, published on March 5th, which condemned Copernicanism in a surprisingly mild tone, and of a certificate of honor given to Galileo by Bellarmine on 26th May, which contradicts the main document in the Vatican files. Wohlwill, Gebler, and Santillana, have all argued more than convincingly that Bellarmine's inside report and his certificate of honour to Galileo are significantly different. Both these documents, however, indicate how seriously Galileo was taken by the Church authorities. Why? How could he throw the Church leaders into a panic and make them act in such a hurry and in so confused a manner? Where lay the power of that isolated sick man?

There are other points which may be explained by the assumption that some confusion resulted from the hasty proceedings of the Church authorities, and that some of the steps which were taken in a hurry were later regretted. Santillana argues that the wording of the condemnation of Copernicanism is confused to the point of meaninglessness [139]. Moreover, there is a discrepancy between two Vatican reports about Bellarmine's warning to Galileo, and between each of them and the certificate of honor which Bellarmine gave Galileo. Since the discovery of the relevant Vatican documents in 1867, the trial of Galileo of 1633 has often been alleged to be legally connected with Bellarmine's warning of 1616, and consequently much ink has been spilt on the worthless

legalistic issue, which of the three documents concerning the warning is the correct one. The more interesting question, I suggest, is why were the Church authorities in such a hurry?

Koestler's explanation of the turn of events rests on a subtle error: he uses the implausibility of his own explanation of the Church's condemnation of Copernicanism to explain the Church's (alleged) subsequent attempt to forget this condemnation. This is the same kind of offence as that committed by a contractor who bills you both for a shoddy job and for the inspection and repairs of its defects. Galileo's bad temper and ability to annoy people, says Koestler, caused the move. These, obviously, could not move even a Pope, let alone a Vatican office, to a rash condemnation of an important doctrine. Therefore, says Koestler, the condemnation was soon buried.

Santillana's story is exactly the opposite. The Church was going to condemn Copernicanism—we are not exactly told why—and all that Galileo wanted was to prevent a rash action. Santillana himself admits in a way that Galileo was the person who had started the affair. Galileo wrote to Kepler in 1597, telling him that he had new proofs for Copernicanism (his law of inertia, I suppose) but would not publish; Kepler urged him, in reply, to publish—in Germany if Italy was too intolerant; Galileo remained prudent enough to say nothing until 1610. "Now [1610] that certainty [of the Copernican doctrine] has been reached", says Santillana, "the motives for silence that he had explained to Kepler no longer were valid" [14]. All that Koestler has to do in order to criticize Santillana is to explain to his reader three points: first, what were the motives for silence (Koestler's answer being, Galileo's fear of criticism and ridicule); second, what was the certainty (that Aristotle was wrong, not that Copernicus was right); and third, what was Galileo's campaign (not to "build up a tidal wave of opinion" [15], since the wave was building up too rapidly anyhow, but, says Koestler, to force his opponents into public admission of their errors and into public acceptance of the new and as yet unproven doctrine).

Santillana does not explain cogently why Galileo started his unfortunate campaign. He argues, with the aid of documents [135], that the softening of the blow to Galileo in 1616 resulted from the strength of Galileo's (theological) *Letter to the Grand Duchess* written shortly beforehand. Yet, clearly, this letter, and its earlier version, the *Letter to Castelli*, had provided Galileo's enemies with the grounds for attack, and had made Bellarmine an enemy. When he describes how Galileo started the campaign, Santillana entirely ignores the Tuscan ambassador's letter of December 5, 1615, in which he is very opposed to the campaign; but when discussing its failure, he agrees with the ambassador [117] that in December 1615 Rome was "no place to come and argue about the Moon". This is terribly apologetic. It is very hard to

piece Santillana's exciting story into a simple pattern of explanation. When it emerges, it seems incredible: though Galileo started a campaign, his opponents are blamed for having started a counter-campaign. Perhaps the methods of Galileo and all his allies were more honest than those of all his opponents (this has not been shown); in any case, Galileo was endangering his opponents' faith, their social and political positions, and even their personal security. Santillana's siding fully with Galileo and against his opponents is a bit hard to endorse.

Santillana's uneasy feelings seem to show clearly, for instance [53], when he explains that Galileo's *Letter to Castelli* was written in reply to Bellarmine's views as expressed in a private discussion with a priest who was a friend of Galileo. As Galileo did not accept all challenges, this is an unsatisfactory explanation. Moreover, it is incredible luck for Galileo that of all anti-Copernican passages in the *Scriptures* Bellarmine should quote in that private discussion *Psalm* 19, knowingly giving Galileo a chance to expound his neo-Platonist light-metaphysics (as Santillana himself notices [154], especially since Bellarmine thought he had much stronger biblical ammunition, viewing King Solomon (*Ecclesiastes*) much greater an astronomical authority than King David (*Psalms*). This, like other facts, is explicable by assuming that it was Galileo who provoked Bellarmine to discuss *Psalm* 19: the person in position is seldom the more provocative party when he bears no malice and is not looking for more troubles than he already has; especially when he has plenty. (Using *Psalm* 19 Galileo was emulating Pico's *Oratio*).

If we view Galileo as selfish and successful, we cannot explain his campaign and defeat in 1616. Koestler's psychological theory about his obsessiveness, in particular, is refuted by the evidence. If, however, we view Galileo as devoted to science, we cannot explain why he did not fight for Copernicanism in 1611, unless we assume that he shrewdly postponed the battle to a more propitious moment; which makes his battle in 1616 an incredible folly [Gebler, 70–75]. There are two traditions about the case, the hostile, best represend by (Brodrick and) Koestler, and the apologetic, best represented by (Gebler and) Santillana. The easiest way to refute these two traditions is to see how the writers who belong to them struggle with the events leading to, and including, the condemnation of Copernicanism of 1616.

5. THE CHANGE IN GALILEO'S BEHAVIOUR

Koestler's story of Galileo's prudence and selfishness up to about 1612, is hard to ignore altogether. Admittedly, Koestler is mistaken in claiming that the *Starry Message* of 1610 contains no "statement in favour of the Copernican system" [430, 431] (the mistake may have resulted

from reading Drake [85]). Admittedly, Koestler is mistaken in suggesting that the commitment to Copernicanism in the *Letters on the Sunspots* of 1613 is "somewhat vague in form". (In the third *Letter on the Sunspots* Galileo says, "An understanding of what Copernicus wrote in his *Revolutions* suffices for the most expert astronomers ... to verify ... his system" [Drake, 130].) These commitments, nevertheless, are not as clear and bold as his private commitments; and the absence of a sufficiently clear commitment involved another failing, namely inability to make proper acknowledgement to ones who were clearly committed. A courtier who had lunch with Kepler tells Galileo (15 April 1610) about the conversation they had: "He said concerning your book [*Starry Message*] that truly it has revealed the divinity of your talent, but that you have given cause of complaint ... since you make no mention of those writers who gave the signal and the occasion for your discovery, naming among them Giordano Bruno ..., Copernicus, and himself" [Singer, 189]. In order to explain this unacknowledged indebtedness, one has to discuss Galileo's methodology, which is cryptic, and the way he made his famous discoveries, which everyone praises but no one discusses.

Bruno's methodology is perhaps a proper starting-point. In the beginning of his first dialogue in his *On the Infinite* (and in the beginning of the last dialogue in the same book) Bruno makes it clear that he is an apriorist, who, however, does not regard observation as useless; the use of the testimony of the senses is "solely to stimulate our reason, to accuse, to indicate, testify in part; not to testify completely, still less to judge or to condemn." This passage seems to me to be of crucial importance. I do not think that Galileo kept to Bruno's methodology all his life, or that he was clear about his own view of the matter. In his *Dialogue on the Two Systems* (1632), when Simpicio asks Salviati (*i. e.* Galileo) whether he is an apriorist or not, Salviati refuses to answer and even bamboozles his audience [Santillana's edition, 202–3, and Santillana's note there; cf. Wiener, *passim*]. But at least in his early period, and up to his *Assayer* (1623), his following of Bruno is quite conspicuous, and even in his *Dialogues*, and in his method of writing scientific dialogues, he is a Brunist.

The reason for Galileo's strong adherence to Bruno may be found in his early, mechanical works, *On Motion* (1590), and *On Mechanics* (1600), which were published only centuries later. Galileo started by accepting Aristotle's mechanics, continued by accepting Archimedes and slowly used his own Archimedeanism and clear thinking to expel his own Aristotelianism step by step. A few bits of early drafts of *On Motion* are published in the English edition, which show how slow was his progress. Nor was the process finished by 1600. Towards the end of *On Mechanics* Galileo develops his own law of inertia in order to explain Archimedes'

screw, yet soon afterwards, when trying to explain what happens when a hammer hits a nail on its head, he employs an Aristotelian theory of impetus rather than his own Archimedean theory. As Lane Cooper has suggested [48 and note], Galileo's early mechanical works should be viewed as part and parcel of a Renaissance attempt to reconstruct both Archimedes' philosophy and certain ancient criticisms of Aristotle. The effort involved was much greater than one may imagine: Archimedeanism was revived before Aristotelianism was thrown out, and the contrast between the two was discovered by a long and arduous process. Since his investigations were primarily conceived as logical problems [Fahie, 19; Koyré, b) Conclusion; Cooper, 48], not as empirical ones, Galileo could hardly be interested in experiments, and this is why he was so interested in methodology and laid such an emphasis on clarity. (Galileo, and to a lesser extent even Bruno, was a forerunner of Descartes in viewing clarity and distinctness—but also simplicity—as criteria of truth).

An impressive example, which Galileo discusses in meticulous detail in the *Dialogue* as well as in earlier works, is this. Everybody (including Leonardo, incidentally) had taken it for granted that a smooth surface reflects light more strongly than a rough one. Consequently, a man on the moon should see the oceans on earth as brighter than the continents. Only clear thinking, says Galileo, can show this to be an error; no amount of experience with walls and mirrors has helped to eradicate it. And a corollary from this correction is most important since it confirms Copernicanism by showing that the moon, being bright, is a rough surface like the earth, not a crystalline body made of the pure fifth essence. Hence we may expect confirmations of Copernicanism to come more easily from clarity than from experience.

With this in mind we can easily understand Kepler's complaint. Galileo's *Starry Message* contains discoveries which confirm Copernicanism: the mountains on the moon and the moons of Jupiter. To this one should add the alleged moons of Saturn and the phases of Venus, which fall in the same category. In all of these cases, there is no doubt, anticipation of the discoveries was essential to making them. As we have seen, Galileo knew, by reasoning alone, that the moon has a rough surface. In his *Starry Message* he describes how he looked for hours with a telescope at a dark spot on the edge of the light part of the moon, until it disappeared, as the shadow on an earthly valley disappears at sunrise. He could not have made such an observation without an anticipation, without, particularly, following Kepler's idea of imagining himself standing on Mars and gazing at Earth. It is hard to exaggerate the significance and novelty of this idea [Einstein, a) 24–5, b) 225]. Kepler's indebtedness to Copernicus for it is obvious; when Galileo imagines himself standing in a lunar valley waiting for sunrise, then, he is indebted to Kepler and Copernicus

at once. Because he was inspired by Copernicus' theory, Galileo took his discoveries to be empirical demonstration of it (like Bruno he did not think that empirical demonstration is complete). That the same holds for the moons of Jupiter and Saturn, as well as for the phases of Venus, and that here Galileo is in debt to Bruno's speculations about the infinity of suns and satellites, is too clear to demand any further elaboration.

Now all this is largely a reconstruction. Galileo himself said little about methodology until after he wrote his *Letter on the Sunspots* (1613), probably because he was still very prudent. Koestler does not quote Galileo's style in this work. Could this be because it is unusually civil, partly even submissive, almost up to the very end? Also the *Letter on the Sunspots* is interesting because it shows that Galileo's earlier *Discourse on Floating Bodies* (1612) had not been intended to arouse opposition, and that when it did arouse opposition Galileo took it lightly, and decided not to answer his opponents, considering himself successful enough in converting judicious people [Drake, 128–9]. This passage Koestler does not quote; rather, to prove his thesis about Galileo's "irrepressible urge to get involved in a controversy" he claims, in his urge[2] to condemn

[2] It is worth quoting Koestler in full on this point, to allow the reader to notice his following three striking errors. (*i*) Koestler himself, though he has published some interesting thoughts about Archimedes, completely fails to restate the contrast between Archimedes and Aristotle. (*ii*) Yet he views Galileo's opponents as unworthy of being criticized. (*iii*) He can be unbelievably unfair to Galileo, to the point of viewing even his (alleged) anticipation of criticism — the thing which every good author does — as a vice. "After his return, in the summer of 1611, from his Roman triumph to Florence, Galileo become immediately involved in several disputes. He had published a treatise on "Things that Float on Water" — a title that sounds harmless enough. But in this pioneer work on modern hydrostatics Galileo had embraced Archimedes' view that bodies float or sink according to their specific gravity, against the Aristotelian view that this depends on their shape. The backwoodsmen were out at once in full cry, swinging their stone axes. They were the more irate as Galileo, instead of letting the facts speak for themselves, had employed his favorite trick of anticipating the peripatetics' arguments, building them up in a mockserious manner, and then demolishing them with glee" [428]. The absurdity of Koestler's condemnation, however, is no excuse for the absurdity of Drake's praise; on the contrary, Koestler's indebtedness shows how easy it is to change the nuance in a passage from absurd praise into its opposite, while leaving the key points untouched. "Shortly after Galileo's return to Florence in the summer of 1611 he found himself once more in collision with the followers of Aristotle... Now they were confronted with a record of experimental data that anyone could verify at will, and the only thing open to question was the matter of interpretation. But in this they were no match for Galileo, whose specialty was the study of experimental results; and even in their own field of constructing ingenious arguments they were hopelessly outclassed for once. Galileo had in fact all their arguments, strengthening these, adding others that had not occurred to them, and then demolishing the whole structure with his own demonstrations and proofs. It was a device which he was to employ extensively in his later works, and one which accounts for his vast influence with nonprofessional readers as well as his extreme unpopularity with the targets of his polemic compositions" [79–80]. This praise is reminiscent of the tragedy of Nijinsky who was hurt by public enthusiasm for his jumps; he wanted to be appreciated as a dancer and not as an acrobat. Drake admires Galileo as an acrobat-polemicist rather than as a teacher of critical thinking. Incidentally, what Drake and Koestler say about Galileo's new experimental facts, about the ease with which Galileo demolished his opponents, *etc.*, are sheer fantasy [*cf.* Fahie, 143–5], as the following paragraphs show.

Galileo, that Galileo raised opposition in writing his *Discourse on Floating Bodies* quite unnecessarily.

A philosopher called Buonamici, who was probably Galileo's teacher in the university of Pisa, discovered (what Galileo did not know when drafting his *On Motion*) that Archimedes' hydrostatic theory belongs to the Platonic tradition of explaining levity (or buoyancy) as caused by the gravity of the medium, and is inconsistent with the Aristotelian tradition of assuming both gravity and levity as essential causes of motion. Consequently Buonamici rejected Archimedes' view. The chief objection to Aristotle's view is that boats made of metal float. This, Aristotle explained away (at the very end of *De Caelo*) by an auxiliary hypothesis about the resistance of water (surface tension), as exemplified by floating metallic needles and thin boards. Archimedes probably refuted this auxiliary hypothesis about floating metal boats. His own treatise *On Floating Bodies* explains why metal boats float, and without any auxiliary hypothesis; but it contains no criticism of any other doctrine. Galileo took up Buonamici's critical mode of thinking; and "speaking always without diminution of his [Buonamici's] singular learning" [22] he refuted his views by reconstructing Archimedes' criticism of Aristotle. The objection to Archimedes' view, however, is the fact that metallic needles and boards do float. Galileo tried to answer this objection. This he failed to do (since the objection is unanswerable). He wriggled out of the difficulties by sheer ability to confuse a simple issue. Following Archimedes, Galileo claimed that the metal needle or board must behave like a metal boat and expel water of a weight slightly exceeding its own. Following Aristotle, Galileo's adversaries denied this. This was the crux of the argument concerning Galileo's views, and he was plainly mistaken. He adduced beautiful empirical refutations of Aristotle; and he was simply convinced that Archimedes' geometrical demonstrations were perfect.

But of course, mistaken as Galileo was concerning his own views, his criticism of Buonamici and his followers is valid. Regrettably, he was careful not to use all the ammunition which was at his disposal, judging by his earlier (unpublished) works. The first half of the *Discourse on Floating Bodies* is a discussion of the criticisms and rejoinders of both sides. The rest of the volume is a reconstruction of a Platonist rejoinder to Aristotle's criticism of the theory that the levity of a body is caused by the gravity of the medium. This goes beyond Buonamici and his followers; it is extremely interesting. Even Drake has to admit [XI–XII] that the only novelty in this book is Galileo's (reconstruction of Archimedes') criticism of Aristotle. In this Galileo was continuing the job which Buonamici started. However, his superiority to Buonamici here was a source of trouble. Buonamici was not enough of a clear and critical thinker to have abandoned Aristotle's views. And in backing Aristotle, who was the accepted right horse, he was allowed by the public to use

logic alone. As Galileo was backing Archimedes, the socially unaccepted horse, the public forced him (by appeals to his employer, *etc.*) to produce not logic but experimental facts. This pressure he may have anticipated, and this anticipation, plus his prudence, may explain why he was silent for years. Afterwards, when he had become famous, he published only a small part of his criticism of Aristotle, and while speaking of him with great civility, saying (untruthfully, I think) "he hath exquisitely philosophiz'd" [64]. But the small dose of criticism was enought to arouse dangerous opposition, of the kind which Drake and Koestler ignore, but which can be judged from the following event. Father Grienberger, a Jesuit astronomer of the Roman College, to whom Galileo referred in 1615 as to "that excellent mathematician and my very dear friend and patron", wrote in 1614 "to a close friend of Galileo to say that were it not for the deference which by the direction of his superios he was obliged to show towards Aristotle, he would have spoken his mind clearly on the matter, in which Galileo was perfectly right" [Brodrick, 347; Santillana, 118n; surprisingly, neither gives any reference]. (Incidentally, Father Grienberger seems to have remained a friend to the last; but he could hardly be of any help to Galileo, it seems, because of his vows of obedience.)

The importance of the early (1612 and before) mechanical works of Galileo lies in his realization of the importance of logic; his considering criticism and clarity to be essential for scientific discourse. Quarrelsome as he could be, he confined his great discoveries prior to 1610 to a small inner circle, because, like most quarrelsome people, he knew with whom not to quarrel. But then, in 1610 fame had been achieved, and he found that ideas mattered to him more than wordly position. He sends a feeble feeler in the form of *On Floating Bodies* in 1612, and soon finds a wall of silence. From now on he becomes reckless and decides to take the bull by its (theological) horns not withstanding any risk to his own position. Koestler's prudent Galileo ceased to exist in 1614–5, and the reckless Galileo had an entirely different character from the one which Koestler ascribes to him. In short, the young Galileo was quarrelsome but prudent; the Galileo who got into trouble with the Church was a changed man.

6. GALILEO'S FAITH AND FATE

It is hard to imagine how much of science was a mere dream at first. That much of it was (pseudo) Pythagerean light-mysticism, has slowly transpired through works of E. A. Burtt and others. That Pythagoreanism was deeply linked then with Cabbalism and alchemy has been shown by a few scholars, such as Blau, Gombrich, and Miss Yates. How much

of the methodology of the time was a mystical dream is a story which has not yet been told. [3]

In Galileo's days, hypotheses—say Ptolemy's—were claimed to encompass all known relevant phenomena, but not to describe any reality; also, usually they were over-complicated. These two points were connected: reality was always assumed to be simple, whereas the appearances were known to be complicated; hence a complicated hypothesis could be posed only as a means of calculating empirical results, not as a truth about reality. Today the term for such a hypothesis is "a working hypothesis"; traditionally, such a hypothesis was called "a mathematical hypothesis" or "a mere hypothesis" [Popper, 168]. One of the reasons for not suggesting hypotheses about reality was, no doubt, the idea that Aristotle had said everything about reality (though the relations between reality and appearances must then be admitted to remain obscure). The other reason for giving up attempts to describe reality, according to Popper's suggestion, was the acceptance of Plato's and Aristotle's idea that statements about reality must be demonstrable (methodological essentialism), plus the realization that demonstrations were inaccessible [78ff, 151].

In an interesting preface to his *Three Copernican Treatises* Edward Rosen discusses Copernicus' use of the word "hypothesis". He argues that Copernicus (and Kepler) denied that Copernicus' hypothesis was a mere hypothesis or a mathematical hypothesis—it was a demonstrated hypothesis. Its being demonstrated was the same as its not being mathematical: there was no other known alternative. That it was demonstrated was shown by its simplicity. Here comes Koestler's very important point, which may have been known all along the way, but which (as Santillana's and Drake's fury illustrates) was never driven home so well:

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[3] Wolfson sees a continuity of method from Antiquity to date [a) 25; b) 106 ff], whereas Popper considers the Middle Ages as the outcome of the death of the Greek methods and the Renaissance of science as their revival [151]. The scholastic methods, so characteristic of the Middle Age , can be traced to the Talmud, Philo, early commentator's on Aristotle, and Aristotle himself. The scholastic method is the critical method as employed elsewhere, in Antiquity or in modern science and scholarship, but with the proviso that the fundamental tenets remain unchallenged plus the technique of inventing *ad-hoc* hypotheses in order to protect them. The criticism of the fundamental tenets was offered by the mystic irrationalists who thus entrenched the identification of rationalism with Aristotelianism. Moreover, since the content of their criticism was identical with (and borrowed from) parts of the Aristotelian commentaries, what distinguished them was their method: by forbidding *ad-hoc* ripostes they turned innocuous flashes of debates into deadly hits. This may explain why the principle of simplicity was of such great methodological import and mystical excitement at the same time (and violating it was so sinful). The peak of the mystic criticism is achieved by Al Gazali and Crescas [Wolfson, a) 11ff], both of whom view their attack on Aristotle as an attack on rationalism. Crescas, however, being a Jew (and thus an adherent to the commandment to study the Law), finds a limited role for reason. The fact that criticism rises in the Renaissance together with mystic irrational cabbalism or Pythagoreanism or Neo-Platonism (*e.g.*, Boccaccio, Pico, Cusanus) is thus no accident. [See also Yates, b) especially conclusion.]

hard as he tried, Copernicus failed to show that his simple hypothesis accounts for the known facts without adding to it many epicycles which render it very far from simple. This may explain his immense reluctance to publish: his idea of simplicity was a dream; not merely a programme which he could only hope to accomplish but which he never did; it was the illusion that the programme had been (nearly) accomplished. But criticism and clarity were integral parts of the dream, and so the Copernicans had to criticize their own views even after they had claimed that these were the truths about reality, and hence demonstrated, and hence clear and simple. The contradictions and non-sequiturs here should delight Koestler. That Kepler had deceived himself in this fashion he tells us, but that Galileo could also deceive himself thus he denies. He scolds Galileo for consciously deceiving people when talking about the circular planetary orbits of the Copernican system, for not caring whether he was speaking the truth in that instance, and for being obsessed with circles.

Let us allow the accused to speak [Drake, 262–3, Drake and O'Malley, 279]: "it is not I" says Galileo "who want the sky to have the noblest [*i. e.* circular] shape because of its being the noblest body ... Never having read the pedigrees and patents of nobility of shapes, I do not know which of them are more and which of them are less noble, nor do I know their rank in perfection. I believe that in a way all shapes are ancient an noble"—which is an explicit commitment to Plato's doctrine of Ideas, from which Galileo shrinks at once to an almost positivist attitude: "or, to put it better [*sic*], that none of them are noble and perfect, or ignoble and imperfect, except in so far as for building walls a square shape is more perfect than the circular, and for wagon wheels the circle is more perfect than the triangle." And yet in the same work (*The Assayer*, 1623), he claims [241 or 197] he would accept no path for a heavenly body save a regular one, such as a circle, a spiral, or an ellipse! Clearly quite a few exciting ideas interplay here, and Galileo himself is tossed between them. That he was aware of Copernicus' epicycles, and worried about them, is also clear from *The Assayer* [264], where he is very proud of having disposed of Copernicus' socalled third motion of the earth. Like Copernicus and Kepler, he has both demonstrated the hypothesis already, and is also going to complete the demonstration pretty soon. For, obviously, demonstration *is* the same as getting rid of all epicycles! This is not our idea of demonstration, but it was his: Galileo both thinks that the epicycles had been eliminated, and that he would be able to eliminate them pretty soon—more by intellectual ingenuity than by observation. It is hard to believe that the greatest logical mind of his age, and the father of scientific method, could think thus; but we should remember that such thinking occurs already in Copernicus' and Kepler's various works, as well as in Galileo's

own *On Mechanics* (of 1600), when he had no possible vested interest in his blunder. It merely comes to illustrate Galileo's own point of how difficult it is to avoid inconsistencies.

Koestler contrasts the ancient Phythagoreans with modern scientists: they lived blissfully before the split between Faith and Reason (which Galileo brought about), and the modern scientists live in a world of strife, in a divided house of Faith and Reason. He views Pythagoreanism as a healthy mixture of Faith and Reason. But he scolds Galileo for his meddling in theology! Not only did Galileo declare himself openly a member of the new Pythagorean movement—his faith in science itself was a kind of religion (and still is with most of us); the Inquisition referred to him as a Pythagorean, and Bellarmine viewed him as a religious reformer. And rightly so. And with the zeal of reformers he fought, and took risks. Koestler thinks that the reform might have been implemented from within (by the Jesuit astronomers, chiefly), and that Outsider Galileo only spoiled matters by iterfering and by annoying the Jesuit astronomers until they became staunch anti-Copernicans. Yet Koestler has given not a single piece of evidence for the view that the Catholic Church has ever been reformed without a bitter struggle. And though we may easily understand, and need not resent, the official theologians' resentment of Galileo's theological writings, it is rather hard to understand Koestler's wery similar resentment of the same. The way he overlooks the fact that his darling Kepler was engaged in similar theological exercises is a serious case of bias. Had Galileo's theology been accepted by the Church directly from him, he might have become a saint, rather than Bellarmine. There was a chance that this would happen, both in 1616, when Bellarmine got into a panic, and after the death of Bellarmine and of the Pope, when the new Pope, Urban VIII, encouraged Galileo to write his *Dialogue*. But even if he had no chance against Bellarmine, his sincerity and courage, as well as his important contribution to Catholic theology, ought to be appreciated (and may be appreciated in the future, even by Rome; remember Joan of Arc!).

Let us glance for a moment at Galileo the Catholic reformer. In his *Letter to the Grand Duchess* [Drake, 181] he offers his view for the Church to consider, he presents it neither as the known truth nor as a point of public debate. Yet the point is that we ought to separate theology from astronomy so as to enable free critical discussion amongst astronomers. And he comes dangerously closely to Brunos' position, for which Bruno was burnt, and at least he attacks (Bruno's judge) Bellarmine quite clearly along Brunist lines: "I question the truth of the statement that the Church commands us to hold as matters of faith all physical conclusions bearing the stamp of harmonious [i.e. unanimous] interpretation by all the Fathers of the Church. I think this may be an arbitrary simplification of various council decrees by certain people [Bellarmine] to favour their

own opinions" [203]. We may remember that Bruno was willing to recant only after an argument with the Pope, not on the authority of his judges (amongst whom was Bellarmine). Koestler asserts that Bruno, a metaphysician rather than a scientist, had nothing to do with Galileo's case, and that when Galileo stood before the Inquisition in 1633 "he was afraid". Galileo's words which I have quoted, written fifteen years after Bruno was burnt on the stake, do not seem to be the words of a coward; one may fail to notice the similarity between Bruno's and Galileo's scientific views, but hardly their religious views in general, and their submission to the Pope cum defiance of the Inquisition in particular; one may dislike their submission to the Pope, but one must admit that they were brave and sincere Catholics, even though the Catholic Church cannot as yet admit this.

To conclude, Koestler expresses more than once his desire not to be wise after the event, but he is wise after the event in Galileos' case, at least. He applies hindsight when he applies what he (erroneously) thinks is the proper criterion for judging whether Copernicanism had been demonstrated, instead of looking for Galileo's own criterion. And he applies hindsight when he takes it for granted that Galileo was bound to lose his theological campaign for Copernicanism. Galileo had a very good chance of winning it; but the point to stress is that we ought to investigate whether he had a good chance, and, what is more important, we ought to notice that he thought he had a good chance to win the battle. (Indeed, at one point he thought the battle was already won [*cf.* Gebler, 177].) Trying to explain his behavior thus may be more interesting than viewing it as irrational, as Koestler does, by saying that he was obsessed with his need to quarrel regardless of the consequences. In brief, Koestler does not attempt a rational reconstruction of the battle as it appeared before it was over.

7. THE *DIALOGUE* AND THE TRIAL OF GALILEO

The most difficult part to reconstruct is the way in which a battle can be fought from within. If one rejects an important doctrine, one becomes an outsider; and if one does not reject it first, one does not wish to fight for its official rejection. Does not one's attempt to alter the official doctrine show one's conviction that it is false? Indeed, it is universally assumed that Galileo did not believe what he was told (by Bellarmine) to believe; defenders of Galileo, like Santillana [e.g., 151], view his professions of faith as ironical, and Catholic apologists as hypocritical. Now (being an agnostic and a Jew) I am a person poorly qualified to explain the fact that Catholics are permitted by their Church simultaneously to believe in a doctrine and to criticize it; yet I wish to state categorically

and most emphatically that such is the case, no matter what is the doctrine in question, no matter how far-reaching are its consequences. After Galileo's *Dialogue* had raised a scandal but before it led to the trial, Galileo's enemies criticized Galileo not for his defence of Copernicanism; this, for all they knew (though not according to the Vatican files), was permitted. Nor did they show that his defence was invalid; Koestler's attribution of such capabilities to them cannot be accepted without argument, and this he does not provide. They criticized him for having allowed himself to believe in Copernicanism on the strength of having demonstrated it and in spite of his having been told not to do so. In other words, they did not deny the validity of his demonstration, they did not deny his right to demonstrate Copernicanism, they merely denied his right to believe Copernicanism on the strength of valid demonstration. In a letter to Galileo from Campanella, Galileo is told that he can safely deny this allegation [Santillana, 191]: "Please note" advises kind Campanella to Galileo when notifying him about the oncoming storm, "that you may hold that ... [Copernicanism] was properly forbidden, without having also to believe that the reasons alleged [by Bellarmine] are good. This is a theological rule, and it can be proved" etc. Not "I believe it because it is absurd", but "I believe it although it is absurd—until I am told not to", is the theological rule; and it is a rule which Galileo did follow, while arguing that the object of belief was absurd. Both Santillana and Koestler overlook this when charging him with cynicism or insincerity.

My contention, that from 1616 onwards Galileo believed in the immobility of the earth, may be false, of course, but for a methodological reason it ought to be investigated first, especially as it explains phenomena which students of the case were puzzled about. The methodological reason is this: it is easy to attribute any motive to any person, and less easy to test such attributions; so we should normally take a person's expressed motives seriously until we can show that he did not speak the truth. Of course, when we have before us, say, the documents which Butler caricatured in his *Erewhon Revisited,* we have to reject the claim he made in his preface, that he had no intention of caricaturing Christianity [Henderson, 220]. And this should make us suspicious of all expressed motives, much more so in the case of Galileo who suffered religious persecution than in the case of Butler who did not. Yet as long as there is no evidence against Galileo's confessions, they ought to be taken very seriously as possibly true; and the same ought to be said of Pope Urban VIII, and of all the others involved. It is quite possible that both Galileo and Urban VIII were irresponsible rascals, as Koestler asserts; but this should not be our starting-point.

Let us glance at Galileo's preface to the *Dialogue* with the intention of believing him for a while. There he speaks "of the Copernican hypoth-

esis, as if *it were to prove absolutely victorius*". This preface, Santillana comments, "was practically dictated to Galileo by his anxious friend Mons. Riccardi, the Master of the Holy Palace, who had been entrusted with the *imprimatur*" [6n], the friend who was known, Santillana says, for "his immense girth and erudition" [170] but whom Koestler views as an ignorant fellow whom Galileo bullied to grant the *imprimatur* without knowing what he was doing. Galileo bullied him indeed, for he was very apprehensive; but, ignorant as he was of astronomical matters, he was quite a theological authority (*cf.* the *Catholic Encyclopedia*); and as he considered the preface to be theologically satisfactory, it is difficult to see why Koestler should not accept this judgment. (The accusation concerning the preface was, originally, merely that it had been printed in a different type from the rest of the book [Santillana, 211].)

Simplicio, the Aristotelian in the *Dialogue,* says Koestler, is "the clown who is kicked in the pants". This is untrue. Even Santillana has commented on Simplicio's charming character, though he also says (*Dialogue,* xxxv): "As to Simplicio, it is reasonable that he should remain under an ancient pseudonym, for his name is legion. He is the average ... Aristotelian professor of the universities." This is considered to be a very important question, as it was alleged that Simplicio was a caricature of the Pope, and as it was also alleged—by Galileo (in a private letter)—that this allegation is the one that led to the trial of 1633. Now Koestler has not the faintest doubt that Galileo was lying when denying that Simplicio was a caricature of the Pope. But Galileo's preface says this. "In the company of [his friends Sagredo and Salviati, after whom he has named two characters in the *Dialogue*] ... I often discoursed of these matters before a certain Peripatetic philosopher, who seems to have no greater obstacle in understanding the truth than the fame he had acquired by Aristotelian interpretations," and who is the source of Galileo's Simplicio. So "Simplicio" is the pseudonym of a real friend of the three who had "often" argued with the three friends, and who was alive in 1632; he was neither a clown to be kicked, nor the Pope. I wonder who he was.

Assuming that Galileo had not violated the letter of Bellarmine's instructions as described in Bellarmine's own certificate of honor to Galileo, and assuming that Galileo had no intention of caricaturing the Pope, we can solve a number of problems. The story of the writing and of the publication of the *Dialogue* becomes amply clear and reasonable if we do not use hindsight, if we forget the ensuing catastrophe when trying to reconstruct the way Galileo, the Pope, and others, looked at the situation before they knew of the grave consequences of all they did. (Even the fact that they were apprehensive, all of them [Santillana, 216n], indicates that none of them acted unreasonably.) But it all becomes clear only when one assumes their intentions to have been honorable. An important factor in the matter may also be the possibility that some mea-

sure of secrecy surrounded the whole scheme; for we know that some Jesuit astronomers were taken by surprise when they saw the *Dialogue* on sale. It is quite possible that the Pope wanted to present them with an accomplished fact.

In the *Dialogue* Galileo says quite often that he does not believe in Copernicanism, but that soon the Pope would permit believing it, and then he gladly will do so. It is difficult to see how Galileo could have been lying, or even merely guessing, when Riccardi was the censor who read the *Dialogue;* for whatever may be said about his comprehension of astronomy, this he certainly did understand, and whether it was the truth or not he most definitely could and would examine—he could, after all, ask the Pope himself.

The whole situation points clearly at the suggestion that the people involved knew what they were doing—they were too cautious and apprehensive not to—and were acting in good faith and not from any personal motive. "If corroboration is needed, it is to be found in ... reports" written by the Tuscan ambassador of the period, concerning the events which occurred between the publication and before the trial. "They stress that [Pope] Urban 'was so incensed that he treated this affair as a personal one', and quote Urban's 'bitter remarks' that Galileo had deceived him." This is Koestler's statement [483] in support of his thesis that the case was a personal affair, whereas the ambasador says the Pope had treated it as [if it were] a personal one—because of its importance. As to Galileo having deceived the Pope, the deceptions explicitly mentioned in the letter are that Galileo did not follow instructions with rigor, which is always a matter open to a difference of opinion, and "that all is well" [Santillana, 192], which, (alas!) turned out to be false. And, it transpires, the Pope feared that the *Dialogue* "might bring religion very great prejudice": the success of the *Dialogue,* if acknowledged, would presumably lead to taking the authority of the Church lightly. Moreover, this Tuscan ambassador, far from trying to prevent Galileo from coming to Rome, just asks the Pope to have a discussion with Galileo; but the Pope is apprehensive, which is very understandable. A trial was much safer than a personal interview.

One of the most significant arguments against Koestler, and all the Catholics he follows, is Gebler's evidence [173, 177] that Galileo was very surprised that his success turned into failure. Moreover, being a faithful Catholic, he cooperated with his interrogators from the start, like Bruno before him, even though to begin with he (Galileo, like Bruno before him [cf. Yates, b) 205 and note]) did not know what it was all about. Even his interrogators had to admit that from the start he behaved "like a good Catholic". But he refused to lie, and so there was an impasse. The inquisitor then suggested to Galileo in a discussion (the inquisitor received, as he says, a special permit from the Pope to take such a "bold step" as

to argue instead of to interrogate), and Galileo soon agreed, that perhaps inadvertantly he said things he never meant to say, and that he was probably vain in placing so much significance on his own original arguments in favour of Copernicanism. This is all: he refused to deny the strength of others' arguments in favour of Copernicanism; nor did he admit that (demonstrated as he thought it had been) he ever believed it after 1616. Koestler nowhere supports his claim that Galileo was lying and could easily have been broken; and Koestler's "understanding" of Galileo's having been afraid is therefore rather uncalled for.

But what was the catastrophe? Why did Galileo's and Pope Urban's plan misfire? Why did the Pope decide to have a trial and humiliate Galileo? Even Santillana, who has an admirably balanced and detached attitude towards the trial, confesses that he fails to understand why Galileo had to be humiliated [301]. My hypothesis is that after the long silence about Copernicanism (1616–32), and with no preparation, and with the public viewing Simplicio as "the clown who is kicked in the pants", it looked as if Galileo was defeating the Church itself, not the Bellarmine sect in it. And, quite possibly (following Galileo's letter about the source of the trouble), Galileo's enemies successfully spread the rumour that Simplicio was the Pope; and then, even though the Pope knew it not to be true, he had to do something about it: this was not a period in which jokes against the Pope were harmless to the Church. So the Church had to assert its authority against Galileo. There is ample evidence for this hypothesis, which I do not wish to marshal; all I wish is that it be examined seriously.

Koestler suggests that Galileo's evidence for Copernicanism was so weak that his bluff was obvious. There is no evidence to support this suggestion. Indeed, Galileo's claim that all the planetary orbits were circular in agreement with observations is very weakly argued, as we know today. But this was not so obvious then. And as to Galileo's own original arguments in favour of Copernicanism, which in his trial he admitted to be less conclusive than he had thought, they were not as weak as Koestler suggests. The first, concerning the sunspots which show that the sun is tilted, is a criticism of the old doctrine of the crystalline spheres. Koestler claims that it does not demonstrate Copernicus' doctrine, as it does not refute Brahe's, and censures Galileo for ignoring Brahe altogether. The reason for this is simple: according to Koestler any doctrine which saves the phenomenon should be tested seriously; according to Galileo however, this is not so: as he explains at length in his *Assayer,* there is nothing in Brahe's system to recommend itself save some errors in mechanics and some errors in theology: it has no metaphysical foundations, and thus it does not count!

Galileo's second argument for Copernicanism concerns the tides. Galileo tried to account for the tides by using his law of inertia, rather than by using a theory of force which would be more in agreement with an idea which Kepler once tossed up. Koestler criticizes this theory first by claiming that the law of inertia cannot explain the tides, and second by pointing out that even Galileo only contended that one tide per day was thus explicable whereas there are two tides per day. Now the first criticism is valid only if the initial conditions are these: the waters are at least during one moment at rest relative to the continents. Whereas Galileo's point was that the initial conditions were different from those of relative rest. This makes nonsense of Koestler's idea about the mathematical connection between Galileo's theory of the tides and the missing stellar parallax, as the initial conditions are so different in the two cases. It also shows that Koestler has missed Galileo's great idea (and its root in the metaphysical theory of simplicity): Galileo claimed that the tides should be explained not by a specially designed universal hypothesis, but by the existing mechanics plus a hypothesis concerning initial conditions, namely plus a model. And he drew great encouragement from having got some results this way. Moreover, Galileo's own (pendulum) model was the starting point of Newton's researches which led him to his theory of universal gravity, and thus it is historically very important indeed [Turnbull, 301]. Furthermore, Netwon followed Galileo in trying to explain the tides not by a new universal hypothesis but by a model. Newton's model was criticized by Laplace on grounds similar to those of Koestler's second criticism of Galileo, and almost as vehemently [Todhunter, § 807ff]. Laplace's own model, incidentally, has meanwhile been rejected as well, and again as one which is not even a tolerable approximation to known facts. And this shows how dangerous it is to be wise after the event and be indignant about the shortcomings of our predecessors. Quite possibly, Galileo was too humble in confessing that he had been too vain when thinking highly of his theory of the tides.

But this is not to commit myself to Galileo's methodology or physics. Though nearer to the truth than its predecessor, Copernicanism is false: the sun is not the immobile centre of the universe. And scientific hypotheses are either tentative or refuted; thy are neither "mathematical" nor demonstrated. But Galileo's views on criticism and clarity and on freedom of thought are admirable, and so was his battle for these, which he lost neither entirely due to his own character nor entirely due to his opponents' malice or stupidity. It was touch and go. It is a great pity that he lost the battle, even though in the long run his ideas were taken seriously both by the world of science and by Catholic theologians.

8. CONCLUSION

"We should be grateful" says Copernicus [Rosen, 93] "... to those who have spoken incorrectly, because to men who desire to follow the right road, it is frequently no small advantage to know the blind alleys." This holds for Koestler too. Although most of his points are not essentially new, it is the first time that a non-Catholic has asserted them, and with such force and vividness. Even Santillana's Geblerian *The Crime of Galileo,* which wonderfully conveys the electric atmosphere of the times, and has in it crowds of real people made of flesh and blood, has for its Galileo and Bellarmine cardboard figures with little cardboard wings instead of real people. Koestler is not half as scholarly, he has no atmosphere, and his Bellarmine is the cardboard figure borrowed from Santillana (borrowed from Brodrick?), and his Galileo has no family and barely any everyday life; yet his Galileo is nonetheless alive and kicking—especially kicking.

Some commentators have suggested that only his Kepler is alive, and they have explained it by his sympathy for Kepler. This is unfair, not to say highly unimaginative. But alive as Koestler's heroes, Copernicus, Kepler, and Galileo, may be, regrettably they cannot be the true historical figures he wants them to be, because he is too nonchalant in his statements about the working of science to explain what a scientist is supposed to do, and too unappreciative of the difficulties which scientists encounter. The following is an example I chose because it wants little comment. Indeed, it is a collector's piece. "Instead of proceeding by observation and measurement, as the Pythagoreans did," Koestler tells us [108], "Aristotle constructed, by that method of *a priori* reasoning which he so eloquently condemned, a weird system of physics 'argued from notions and not from facts'." Yet the whole of the book, including the passages about "the Pythagoreans", comes to illustrate, Koestler also tells us, "one of the points that I have laboured in this book," namely, "the unitary source of the mystical and scientific experience" [426]—that is, that the process of developing scientific theories is that of mystic intuitions and not of observations and measurements. A person who speaks thus about science cannot be expected to do justice to the difficult problems involved in Galileo's method and methodology. It has not even occurred to him that to Galileo scientific demonstration is "geometrical" in the sense that Archimedes' *On Floating Bodies* is, and definitely not in the modern sense. This raises problems concerning observation which Galileo could not solve; nobody has solved all of them yet. I know of very few philosophers whose views on observations ought to be taken seriously. Seeing the situation thus, I cannot even start understanding how we can judge Galileo's behavior by the yardstick of whether and when Coper-

nicanism has been demonstrated. All we have to remember, I think, is that no one in modern times had even thought about tentativity before Pascal and Boyle. And even after tentativity had been invented, or re-invented, Newton could not stomach it, so that it did not receive any popularity before the Einsteinian revolution, and before Popper presented a methodology—a false one, I think—based on the true idea that tentativity is an essential feature of all empirical science. But Koestler takes no account of all this, as we can see from his claim that Einstein has not yet influenced man's changing vision of the Universe (not to say his—rather funny—attribution of the idea of tentativity to Kepler). His chief accusation against Galileo, his claim that Galileo ought to have held Copernicanism as a tentative hypothesis, is thus answered. Koestler wants Galileo to have done the right thing, though this even Newton and Maxwell were unable to accomplish. Koestler's chief weakness seems to be taking great ideas for granted (quite against his intentions). In particular, he does not appreciate the greatness and novelty of the various ideas invented by Galileo, and of ideas invented much later, including the idea of tentativity in science.

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