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KOURGANOFF'S CONTRIBUTIONS TO THE HISTORY OF THE DISCOVERY OF PLUTO

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The recent measurements of Pluto's apparent angular diameter by G. P. Kuiper[1] have revived astronomers' curiosity regarding the problems associated with this most distant known planet. Two such problems concern the *discovery* and the *mass* of Pluto. Unfortunately, most modern accounts[2] of the discovery of Pluto or of the determination of its mass do not mention the fundamental contributions to these subjects published in France in 1941 by Dr. V. Kourganoff.

The purpose of this article is to make Kourganoff's work more widely known, and to summarize the simpler and more fundamental aspects of his analysis. For its many ramifications, the reader is referred to the original work. What follows is partially a digest of Kourganoff's memoir, "La part de la Méchanique céleste dans la découverte de Pluton," 1941,[3] and partially a liberal translation of portions of his summary in *Ciel et Terre*, "Nouvelles données sur l'histoire de la découverte de Pluton," 1944.[4]

The discovery of Pluto was in many respects similar to that of Neptune, for several astronomers had calculated a hypothetical planet's orbital elements and mass to account for unexplained perturbing effects on other objects in the solar system. At first

[1] *Pub. A.S.P.*, **62**, 133, 1950.

[2] For example: Russell, Dugan, and Stewart, *Astronomy*, **I**, 406, 1945.

[3] *B.A.*, t. **XII**, Fasc. IV, 147–258; Fasc. V, 271–301; Fasc. VI, 303–41, 1940.

[4] *Ciel et Terre*, **60**, 180–95, 1944.

there was little question of the validity of Lowell's prediction,[5] because (1) Pluto was discovered only 6° from where Lowell predicted;[6] (2) the orbit of Pluto is similar to the orbit Lowell predicted; (3) there was agreement with the less precise graphical method of William H. Pickering, and whereas Lowell used only the residuals of Uranus, Pickering used those of Uranus and Neptune.

About a year after Pluto's discovery, however, E. W. Brown published two papers[7,8] in which he concluded that the calculations of Lowell and Pickering were irrelevant and that "the discovery of Pluto within 6° of one of the two places indicated by Lowell was purely accidental."[9]

Brown's criticisms are essentially indirect; he does not analyze Lowell's or Pickering's work, nor the observed elements of Pluto's orbit. These are Brown's arguments:

1. Because the modern residuals (observed minus predicted longitude) of Uranus are small, Lowell's solution depended mainly on old observations of Uranus (before 1780) with large probable errors. These old observations must be given low weight in the solutions. Consequently, a prediction based on these old observations has no value.

2. Lowell predicted a mass of six or seven earths for Pluto, but the observed value of Pluto's mass is not nearly this large. If the mass of Pluto is anywhere near one-half the earth's or less, then Pluto's perturbations of Uranus, in the period before 1780, are less than the probable errors of the observations.

3. Lowell predicted a conjunction between Pluto and Uranus in 1853 simply because that date is situated near the middle of the modern observations (1781–1903). In the absence of true per-

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[5] A good early account is Crommelin's "The Discovery of Pluto," *M.N.*, **91**, 380, 1931.

[6] Lowell's work gave a double solution; the other predicted position was 180° away.

[7] "On a Criterion for the Prediction of an Unknown Planet," *M.N.*, **92**, 80, 1931.

[8] "On the Predictions of Trans-Neptunian Planets from the Perturbations of Uranus," *Proc. Nat. Acad. Sc.*, **16**, 364, May 1930. See also *Pub. A.S.P.*, **44**, 24, 1932.

[9] See p. 81 of reference 7.

turbations, the hypothesis of a perturbing planet tends to place it symmetrically within the interval where the residuals are small—in this case, the region of the modern residuals.

4. The run of the residuals of Uranus due to the perturbations of a trans-Neptunian planet must have certain general characteristics. But the modern residuals of Uranus do not exhibit these characteristics, and therefore "cannot be explained on the hypothesis of the existence of an unknown planet with any probable mass or at any probable distance and, in particular, by the action of Pluto."[10]

These statements led Kourganoff to ask (1) Why was Lowell's solution determinate? (2) Why did Lowell's and Pickering's solutions agree? (3) Why does Lowell's hypothesized planet (resembling Pluto "accidentally") reduce the systematic residuals of Uranus by 99 percent? The burden of Kourganoff's investigation was to give answers to these questions.

Kourganoff's study of the problem is quite different from Brown's. He divided his memoir into five sections: I. Pluto's Perturbations of Uranus; II. The Work of Lowell; III. The Work of Pickering; IV. Discussion of the Criticisms of Brown; V. General Conclusion.

I. PLUTO'S PERTURBATIONS OF URANUS

The problem of the analysis of Lowell's work is so complex that Kourganoff decided to calculate first the exact perturbations of Uranus by Pluto. Using the best available elements for the two planets, he computed the deviations of Uranus from its orbit caused by the attraction of Pluto. To verify the results of his calculations using the laborious classical method, he also found the perturbations by a modification of LeVerrier's interpolative method. The agreement of the two solutions serves to check the work. The difficulty of the calculations is caused by (*a*) the large eccentricity of Pluto's orbit, 0.25, and by (*b*) the near commensurability of the mean motions of Pluto and Uranus. Figure 1 shows the perturbations in longitude.

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[10] See p. 81 of reference 7.

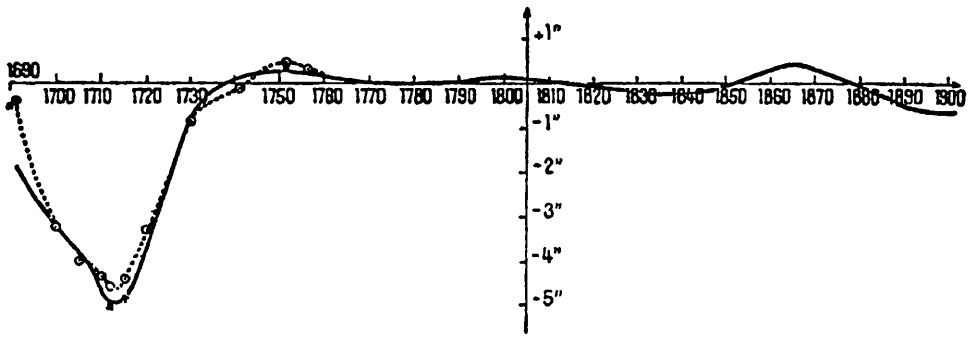

FIG. 1.—Perturbations in the true longitude of Uranus: full line, by the method of interpolation; dotted line, by the classical method. Values adopted in the final equations are indicated by *X*. (This is Figure 2 in Kourganoff's memoir.)

Kourganoff draws the following conclusions:

1. The perturbations before 1780 are strikingly larger than the modern perturbations.

2. The old observations are in better accord with the theory than could have been suspected beforehand, and certainly better than Brown had supposed. Exceptions to this are three observations of 1715 by Flamsteed—the mean is in error by one second of arc in right ascension.

3. The modern perturbations of Uranus are extremely small, in support of Brown's statement, and would not be very useful for research on Pluto.

4. A determination of the mass of Pluto from the residuals of Uranus is possible, and yields a mass for Pluto equal to that of the earth, perhaps slightly larger.

5. Extremely small residuals in the motion of Uranus remain even after allowance for Pluto. Whether these are due to systematic errors or to another trans-Neptunian planet is for the future to decide.

II. THE WORK OF LOWELL[11,12]

Lowell's method is essentially that used by LeVerrier for the prediction of Neptune, but with the following modifications:

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[11] "Memoir on a Trans-Neptunian Planet," *Mem. Lowell Obs.*, **I**, 1, 1915.

[12] Also of interest here is A. Lawrence Lowell's *Biography of Percival Lowell*, New York City, 1935, which includes an appendix by H. N. Russell.

1. In LeVerrier's prediction of Neptune, the distance was determined by Bode's "Law," whereas in Lowell's method solutions were made for various distances.

2. Lowell used least-squares reductions nearly exclusively.

3. His development is limited to the perturbations in longitude.

4. Lowell used a second-order solution because of the large eccentricity of Pluto's orbit.

5. He used as a criterion of the goodness of his solution, the percentage reduction of the residuals of Uranus.

Kourganoff shows that it was the early large perturbations of Uranus (due to a conjunction at the same time as a minimum of approach) which enabled Lowell to make his prediction. And he re-emphasizes the impossibility of evaluating a priori the systematic errors in the old observations. In other words, the assumption that the early observations were precise was taken as a working hypothesis by Lowell.

To see how close Lowell's prediction was, Kourganoff made the interesting diagram presented here in Figure 2. Notice how accurately the predicted orbit of Lowell's trans-Neptunian planet resembles the orbit of Pluto. It is much more significant to compare predictions in this manner than to compare elements directly. LeVerrier[13] said that the elements "are only mathematical *auxiliaries* taken to lead us to a knowledge of the direction and distance, and that these auxiliaries could vary considerably without ceasing to give, at the epoch of the perturbations, the position of the perturbing body." Notice also the fact that two successive conjunctions are *not* equivalent, a consequence of the high eccentricity of Pluto's orbit.

Lowell's calculation led to double solutions for the position of the trans-Neptunian planet. The situation is somewhat the same as if we tried to predict the position of the moon from observations of the lunar tides. Lowell's two best solutions, diametrically opposite, reduced the outstanding residuals of Uranus by 99 percent and by 90 percent, respectively. In Lowell's opinion, this difference of 9 percent was not large enough to distin-

[13] Quoted in Kourganoff's memoir, p. 219.

guish between the two.[14] Pluto was actually found in the position which gave the 99 percent reduction.

Lowell, like LeVerrier, was faced with the questions: What is the accuracy of the prediction? How do the errors of theory and observation contribute to the "probable error" of the predicted position? LeVerrier assigned, more or less arbitrarily, a "probable limit" 5° behind and 18° ahead of the predicted position for Neptune. Actually Neptune was found 52′ from the predicted place!

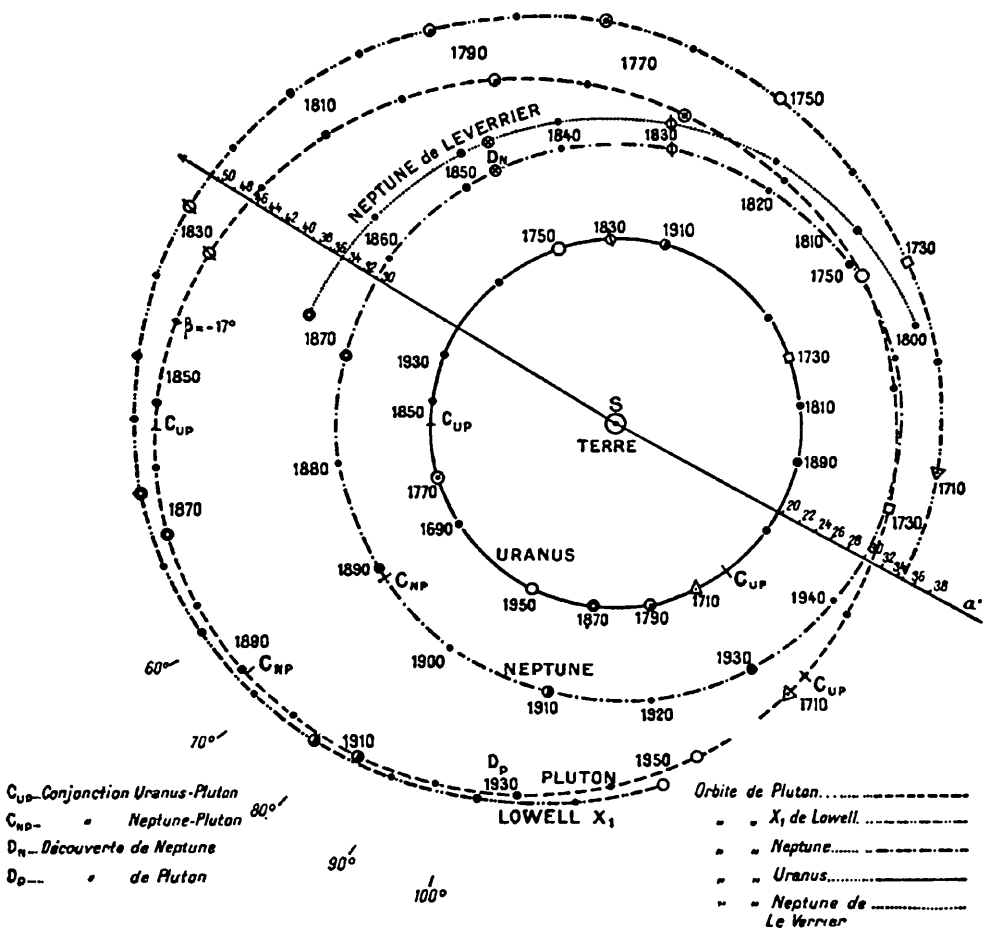

FIG. 2.—The orbits of the earth, Uranus, Neptune, and Pluto projected on the plane of the ecliptic. The designation "X_1 de Lowell" refers to the orbit Lowell predicted which gave the 99 percent reduction in the residuals. (This is Figure 3 in Kourganoff's memoir.)

[14] There were additional considerations which appear to have influenced Lowell. See his memoir, p. 103, sec. 68.

III. THE WORK OF PICKERING[15]

Pickering's method was an elaboration of a graphical procedure described by Sir John Herschel.[16] Yet much of Pickering's work had an empirical or arbitrary character. Although his method was crude, he did make the following significant contributions:

1. He developed the idea of a graphical solution.
2. In 1909 Pickering successfully predicted from the residuals of Uranus the next perturbations of Neptune caused by the unknown planet.
3. He used, in 1919, the residuals both of Uranus and of Neptune to predict the position of a trans-Neptunian planet.[17]
4. He removed the duality of Lowell's solution.
5. He successfully used (1919) Neptune's residuals in latitude to predict the inclination and node of Pluto's orbit.

The various predictions are plotted in Figure 3. Observe how the predictions cluster into two opposite groups.

Pickering's 1919 work was followed by an unsuccessful photographic search at Mount Wilson. This was particularly unfortunate, for it seems to have led Pickering to distrust his own prediction of 1919, and to revise it in a more or less arbitrary and invalid manner. As is now well known, four of the 1919 search plates contained unnoticed images of Pluto!

The systematic comparison of the works of Lowell and Pickering shows that while the errors of Lowell's prediction are due primarily to inadequacies in the given *data*, the errors in Pickering's work are due primarily to the inadequacies of his *method*. So, then, to Lowell goes first credit for the "theoretical" part of the discovery of a trans-Neptunian planet, and to Pickering, second place for removing the indeterminacy of Lowell's double

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[15] "The Transneptunian Planet," *Harvard Annals*, 82, 49, 1919; "A Search for a Planet Beyond Neptune," *ibid.*, 61, 113, 1909.

[16] Sir John F. W. Herschel, *Outlines of Astronomy*, chap. xiv, London, 1849.

[17] He unsuccessfully tried to use the residuals of Saturn; see Pickering, "The Next Planet Beyond Neptune," *Pop. Astr.*, 36, 143, 1928.

solution and for confirming Lowell's work by the use of Neptune as well as Uranus.

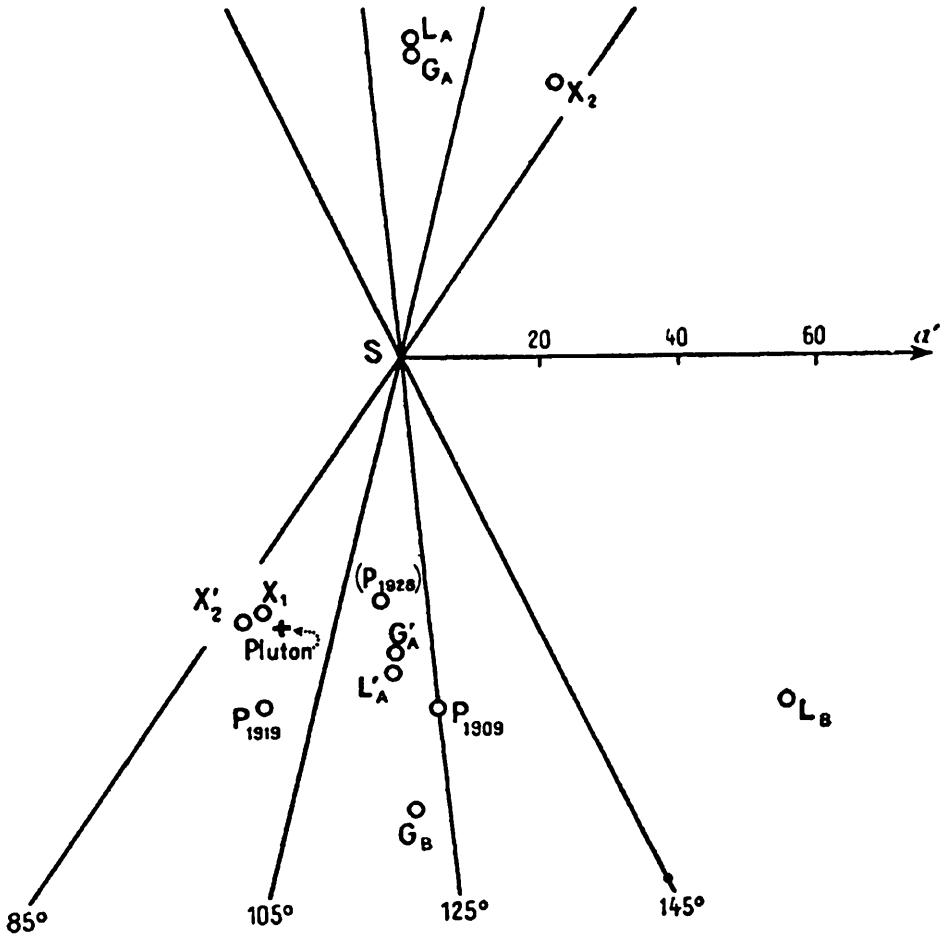

FIG. 3.—Positions of Pluto and "theoretical" planets in 1920.0. The positions are projected onto the plane of the ecliptic. The true position of Pluto in 1920.0 is marked by +.

Symbol	Predictor
X_1 and X_2	Lowell
L_A and L_B	Lau
G_A and G_B	Gaillot
$P_{1909}, P_{1919}, P_{1928}$	Pickering

The primes denote symmetrical positions. (This is Figure 21 in Kourganoff's memoir.)

IV. DISCUSSION OF THE CRITICISMS OF BROWN

This section of Kourganoff's memoir contains a systematic refutation of Brown's arguments cited above.

1. The probable errors of the old observations

Of course Lowell was not able to know, as we do now, how unusually accurate the old observations were. Lowell regarded the assumption of high precision for the old observations as a "working hypothesis." No solution would have been forthcoming without this assumption.

2. The very small mass of Pluto

a) The mass predicted by Lowell is much larger than the "accepted" mass of Pluto.

Lowell's error in the determination of the mass of Pluto is independent of his prediction of the longitude. In effect, the predicted longitude depends principally on the *date* of maximum perturbation, while the predicted mass depends upon the *amplitude* of maximum perturbation. The general form of the curve (see Fig. 1) suffices to determine the longitude independently of the choice of the elements of the elliptical orbit of Uranus. On the other hand, an incorrect choice of these elements could greatly change the aspect of the residuals and completely falsify the mass calculations.

b) Pluto's mass is so small that its perturbations of Uranus are masked by the errors of observation.

This argument implicitly assumes the probable inaccuracies of the old observations. But, as we have seen, the old observations are good, and their errors did not mask the perturbations. Consider also the power of a large number of individual observations reduced by least-squares in revealing systematic trends.

3. The part of the prediction of Pluto based on the interval of the modern observations symmetrical with respect to the conjunction of 1853

Kourganoff considered in detail the validity of this criticism in the light of his study of the inner workings of Lowell's solution and was able to show that this criticism does not apply. Because one may achieve Lowell's result by incorrect reasoning, it does not follow that Lowell's reasoning was incorrect.

The calculations are long and complex, but an example from Lowell's work will help illustrate the invalidity of Brown's criticism. When Lowell used the observations from 1710 to 1903 he found that the longitude of the hypothetical planet was 88° in 1914.5. Incorporating into this group the observations from 1903 to 1910, he found that the longitude was 84° in 1914.5. If Brown's reasoning were correct, an increase of 15° in the predicted longitude should have resulted from this increase of 3½ years in the median date instead of a decrease of 4°.

Brown remarks further,[18] "But there is doubt as to whether a planet can be found which will give small residuals for as long an interval as 147 years and still satisfy the early residuals." Let us refer again to Figure 1, which illustrates precisely this phenomenon. Brown's error arises from a confusion between angular conjunctions and minima of approach. Because of the high eccentricity of Pluto's orbit, the interval between times of minimum approach (252 years) is about double the interval between periods of angular conjunction (126 years).

4. The run of the residuals of Uranus

In order to compare the observed residuals of Uranus with those caused by a hypothetical trans-Neptunian planet, one must first recall[19] that, ". . . the residuals left by the theory are not at all the outstanding perturbations, but only such small part of them as cannot be got rid of by suitable shuffling of the cards. We have then no guarantee that our supposed elements are the real ones, but only the best attainable under the assumption *that no unknown exists*. Every theory of a planet is thus open to doubt, seeming more perfect than it is. It has been legitimately juggled to come out correct . . ."

So this comparison of residuals is not a simple problem. Brown devised a mathematical transformation to apply to the observed residuals which enables this comparison to be made with a minimum of calculation. In Brown's words,[20] "The transformation is used to eliminate the unknown changes in the eccen-

[18] See p. 370 of reference 8.
[19] See Lowell's memoir, p. 4, sec. 3.
[20] See pp. 80, 81 of reference 7.

tricity and longitude of perihelion of the disturbed planet caused by the insertion of a new perturbation. The transformed observations will, in certain cases, show whether a given set of residuals can be accounted for by the existence of an unknown planet or not. It is then seen that, if the hypothesis appears to be valid, a first approximation to the place of the unknown planet can sometimes be immediately obtained."

Brown's transformation may therefore be used both as a *criterion* and as a *method of research* for unknown planets.

Kourganoff shows that Brown's use of the transformation as a *criterion* is unacceptable for several reasons.

1. Because of the character of the transformation, it is not directly applicable to isolated observations, that is, to the old residuals.

2. The assumption of circular orbits is made in deriving the transformation; this is a poor assumption for the orbit of Pluto which has an eccentricity of 0.25.

3. It is not sufficient to transform only the modern residuals because the perturbations of Uranus are sensible only before 1780 and will not begin to be significant again until after 1950–60. Kourganoff modified Brown's transformation and used all of the observations. He found that for both Uranus and Neptune, the transformed residuals (in the neighborhood of maximum perturbations) *do* exhibit a form characteristic of the perturbations of an exterior planet.

As a *method of research* for the prediction of new planets, Brown's transformation may be considered as a method intermediate in labor and in exactness between the graphical analysis of Pickering and the rigorous LeVerrier-Lowell method.

V. GENERAL CONCLUSION

The discovery of Pluto was neither purely accidental nor purely the result of the application of the principles of celestial mechanics. The adoption of a too limited point of view here will entirely distort the truth. In any great scientific discovery there are many factors essential to success. For the discovery of Pluto, they were

1. The verification of the working hypothesis of the accuracy of the old observations
2. The accurate theory of LeVerrier-Lowell which transformed the observations of Uranus, old and new, into something useful
3. The careful search for the trans-Neptunian planet at the Lowell Observatory—the enthusiasm of Percival Lowell.

Kourganoff's conclusion is: The thesis of "pure chance" in regard to the discovery of Pluto is absolutely untenable. Pluto was "discovered" in 1915 by Lowell and "rediscovered" in 1919 by Pickering by the methods of celestial mechanics, before its "physical" discovery by Tombaugh at the Lowell Observatory.

I take this opportunity to remind the reader that this paper is a summary, not a critical review, of Kourganoff's work. From my correspondence with Dr. Kourganoff, I know that although his ideas on Pluto are today fundamentally the same as they were in 1941, many of the statements in his memoir, and in this summary, require slight modification. I thank Dr. Kourganoff for the patient interest he has shown in this paper. I thank Dr. S. B. Nicholson and Dr. L. E. Cunningham for suggesting that this summary be written. I also thank the many others who have read this paper before publication, many of whose helpful suggestions are embodied in it.

February 1951