CHAPTER 3

Some Remarks on the Early History of the Bouguer Anomaly

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3.1 INTRODUCTION

We think that the present-day theory and practice of ground gravity method can be seen in the proper light only when we know as much as possible about its basic elements, namely about the Bouguer anomaly, which we consider *a most important notion*. We think we know how to calculate this quantity, but do we know enough about its historical development and background?

Unfortunately, this topic is rather large and therefore an ambition to draft out the history of such a subject matter, which would be both complete and the same time concise, would be unrealistic. For that reason, instead of providing an overall review, we would like to present some pieces of the mosaic, derived from the earlier contributions of various authors and perceived by us as being important. In both its content and form, this retrospective is a consequence of many discussions among its authors over the years. Needless to say those discussions were always followed by searching for the literature and then searching in the literature, establishing a seemingly never-ending process.

We realize that writing about history should not be confused for writing a critique, yet it will become evident later that some critical approach will have still to be involved. Presumably, one can hardly analyze the development of a subject without touching intimately the subject as such.

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We appreciate that today the majority of the exploration geophysicists understand the Bouguer anomaly simply as "the difference between the observed gravity and the modeled or predicted value of gravity at the station" (Hinze et al., 2005, p. J28, to quote here just the newest of many important papers advocating the concept of station anomaly). In fact here the authors use the term "gravity anomaly" while the terms "Bouguer gravity anomaly" or "Bouguer anomaly" occur elsewhere in their text. At the first glance it is obvious that such a definition is rather vague, but something like this we will have to face throughout all the following parts of our contribution. Moreover, some vagueness or ambiguity will later silently emerge from our retrospective possibly as one of the perceived characteristic features of the Bouguer anomaly.

To keep our text focused on the history (and especially on the earlier history) of the topic we will have to ignore some significant issues as are the influence of the Earth's atmosphere (Ecker and Mittermayer, 1969, and others) or the so-called geophysical indirect effect (Chapman and Bodine, 1979, and others). As well, we will not touch in a greater detail the concept of isostasy (e.g., Watts, 2001) which we rather consider an independent matter. Further, we will not discuss airborne or satellite gravimetry at all, and we will mention the underground and underwater conditions only marginally. We will focus almost exclusively on the ground (surface) measurements of gravity and the Bouguer anomaly calculated on their basis.

3.2 THE EARLY DAYS: GEODESIC MISSION TO ECUADOR (THEN PERU) AND THE BOOK OF BOUGUER (1749)

It is well known that this famous mission commenced in 1735. Three French academics, namely Pierre Bouguer, Charles Marie de La Condamine, and Louis Godin; two Spanish naval officers, Antonio de Ulloa y de la Torre-Giral and Jorge Juan y Santacilia, were the five prominent mission members. On the other hand, the conclusion of the mission cannot be clearly specified because of serious quarrels among its members. For instance Bouguer returned home in 1744. The primary aim of the mission was "measuring an arc of the meridian near the equator in order to compare the corresponding length of a degree with that which had been obtained from the French arc by Jean Picard and by Jacques Cassini" (Todhunter, 1873a, p. 93). They also measured the Earth's gravity at different elevations above sea level by a one-second pendulum.

The most famous and presumably the most important report about the mission results is given in the book of Bouguer (1749). Before we proceed further with our own discussion we would like to give here two citations. The first one is Todhunter's (1873a, p. 248) comment which we quote since we consider his description of the Bouguer's book illustrative: "Bouguer treats on the diminution of attraction at different heights above the level of the sea. He finds that on a mountain at the height h above the level of the sea, the attraction is proportional to

$$(r - 2h)\Delta + \frac{3}{2}h\delta \tag{3.1}$$

where r is the Earth radius, Δ the Earth mean density, and δ the density of the mountain. This is the first appearance of the formula, which has now passed into elementary books." We can only agree with Todhunter that the importance of the expression (3.1) is indeed extraordinary, especially from the aspect of our topic. The corresponding part of the Bouguer's original text is reproduced in Fig. 3.1.

The second citation is a statement of Bullen (1975, pp. 13, 14): "… the second and third terms" in one of Bullen's equations very similar to the expression (3.1) "are associated with what are now, in the reduction of gravity observations, called the free-air and Bouguer corrections, respectively." This statement of Bullen sheds light on the Bouguer anomaly definition as we know it today and on its direct relation to Bouguer (1749). On the other hand, from the aspect of terminology, it is interesting to note that Bullen associates the expression (3.1) with the "reduction" of gravity while he calls its terms Nos. 2 and 3 "corrections."

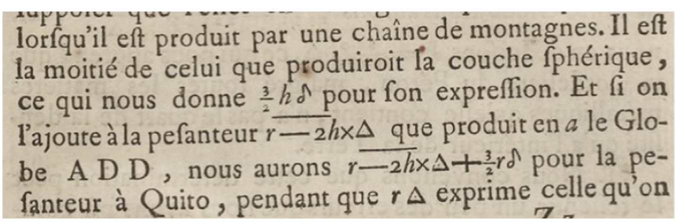
lorſqu'il eſt produit par une chaîne de montagnes. Il eſt
la moitié de celui que produiroit ſa couche ſphérique,
ce qui nous donne $\frac{3}{2}h\delta$ pour ſon expreſſion. Et ſi on
l'ajoute à la peſanteur $\overline{r-2h}\times\Delta$ que produit en a le Glo-
be ADD, nous aurons $\overline{r-2h}\times\Delta+\frac{3}{2}r\delta$ pour la pe-
ſanteur à Quito, pendant que $r\Delta$ exprime celle qu'on

Figure 3.1 The original text with the original expression (3.1) (Bouguer, 1749, p. 361). Please note that horizontal line was used in that time instead of parentheses. Also note that there is a misprint in the resulting formula (there should be h instead of r in the last term). Todhunter (1873a) quotes the expression correctly and does not mention the misprint at all.

The second and third terms can be better seen in expression (3.1a) where we removed the parentheses from Eq. (3.1), just to make it demonstrative:

$$r\Delta - 2h\Delta + \frac{3}{2}h\delta \tag{3.1a}$$

Let us now examine the expression (3.1a) in a greater detail, with some help from Bouguer's own text. First let us multiply all three terms by the factor $\frac{4}{3}\pi\gamma$, where the last symbol stands for the gravitational constant with the known value of 6.674×10^{-11} kg/m^{3}per s^2 (Petit and Luzum, 2010, p. 18). We get

$$\frac{4}{3}\pi\gamma r\Delta - \frac{8}{3}\pi\gamma h\Delta + 2\pi\gamma h\delta \tag{3.1b}$$

It is now evident that the first term of Eq. (3.1b) represents the gravitational effect of a homogenous sphere with radius r and density Δ, calculated at its surface. Taking the γ value as quoted earlier, Δ equal to 5515 kg/m^3 (Cox, 2002, p. 12) and $r = 6371000$ m (the mean radius of the oblate ellipsoidal reference figure, Cox, 2002, p. 240) we obtain for the Bouguer's first term the value about 981938 mGal (1 mGal $= 10^{-5}$ m/s^2). Just as a matter of interest, the modern GRS80 gravity formula (Moritz, 1988, p. 353) results in the value of 981938 mGal for the latitude of approximately 61 degrees. In other words, even if we disregard the Earth rotation and if we use the spherical approximation of the Earth, with constant density, which is rather rough, the estimated gravitational effect falls well within the acceptable limits.

Further we would like to focus on the origin of the second term in Eq. (3.1) or (3.1a). Among others its meaning was correctly understood by Bullen (1975, pp. 13, 14), but the question is how had Pierre Bouguer arrived at it? Bouguer (1749, p. 358) wrote, when commenting upon the shortening of the pendulum length by 1/1331 between an island in the river Inca situated at a low elevation, and the city of Quito, elevated above the lower station approximately by 1/2237 of the Earth radius, that the pendulum shortening was not too far from reciprocal proportionality to the square of the height difference "since we know that squares of quantities which differ only slightly one from another will change twice in proportion to those quantities." We consider this statement as quite essential from the aspect of proper understanding Pierre Bouguer's thoughts. What did he mean? We presume that Bouguer spoke here about the differential $dy = 2xdx$ of the function $y = x^2$ since that notion as well as the mathematical tool had been then already known, thanks to Leibniz and Newton. Thus, if $x = 1$ and $dx = 1/2237$, for the differential in question we will have

$$dx^2 = 2 \times 1 \times \frac{1}{2237} = \frac{2}{2237} = \frac{1}{1118.5}$$

provided that the decrement of the pendulum length would be proportional solely to the increase of the distance from the Earth center. Then his pendulum should have been shorter at the Plaza Grande in Quito rather by 1/1118.5 than the determined rate of 1/1331. Nevertheless, in his evaluation of the expected gravity at Quito he decided to consider the inverse-square principle in the form of the second term in his expression (3.1), and later to introduce the corrective third term. This we regard the key step in Bouguer's thinking. In fact Bouguer did not write explicitly how he got the actual form of the second term but he most likely calculated the ratio

$$\frac{g_Q}{g_I} = \frac{r^2}{r^2 + 2rh + h^2}$$

where g_Q and g_I stand for the quantities proportional to gravity at the stations in Quito and the river Inca island, respectively, and h means the elevation difference between those two stations.

Then, supposing that g_I is proportional to $r\Delta$, he obtained

$$g_Q - g_I = \Delta \frac{-2h - \frac{h^2}{r}}{1 + \frac{2h}{r} + \frac{h^2}{r^2}} \approx -2h\Delta \tag{3.2}$$

after neglecting the fractions with r or r^2 since $h \ll r$ (Bouguer, 1749, p. 360, wrote that h in proportion to r was "très-petite"). Instead of gravity g we could have used pendulum length l to which g is proportional and which was actually measured. Well, the mentioned simplification, especially regarding the term $-h^2/r$ in the nominator, does not seem to be legitimate but, on the other hand, the difference between the approximation $-2h$ and the full fraction in Eq. (3.2) (not considering the multiplication by Δ!) represents only 0.067%, 0.112%, and 0.208% for the elevations of Quito, Pichincha, and Mount Everest, respectively.

According to Bouguer (1749, p. 360) the second term of our expression (3.1a) describes the diminution of gravity when we move from the river Inca island to Quito only if the Earth "would end" at the level of the lower station. All this should change however, if we add a new spherical layer to the Earth with the thickness h. As a result the new (hypothetical) gravity at Quito would have been equal to $(r+h)\Delta$ (provided that the density of the added spherical layer would remain the same), instead of $(r-2h)\Delta$ which we should have measured if there were no rock-filled spherical layer beneath our feet at Quito. The effect of the (complete) spherical layer calculated at Quito would then be $3h\Delta$ provided that the density *was* the same as for the whole Earth. But Bouguer noticed this was not the case and therefore the final form of the spherical layer effect he estimated as $3h\delta$, where δ stands for the density of what we now would call topographic masses. Yet he instantaneously recognized that the Cordillera could not produce an effect comparable to the one of a complete spherical layer. Trying to approximate the true topographic effect, he first considered a roof-like model with the ridge angle of 90 degrees between the roof sides which should produce about 1/4 of the complete spherical layer effect (this Bouguer states without derivation but it can be easily checked; we have found it to be essentially correct; see Fig. 3.2).

Bouguer then appreciated that in reality the base of the Cordillera must be 80–100 times greater than its height, and as a result, the roof-ridge-angle should increase from the previously considered 90 degrees

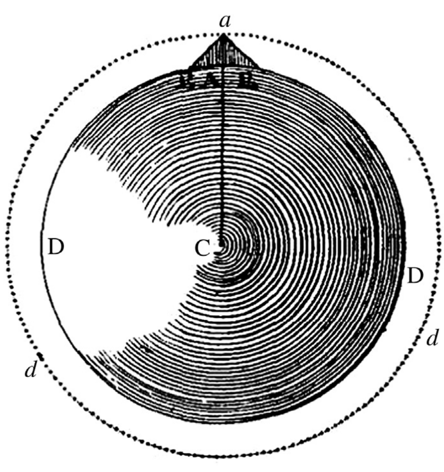

Figure 3.2 Bouguer's estimation of the Cordillera's gravitational effect at the station in Quito, by the roof model. In his first attempt, Bouguer used a roof-ridge-angle of 90 degrees as shown. Reproduced from Bouguer, P., 1749. La figure de la terre. Paris: Charles-Antoine Jombert, 394 p.

to about 170 degrees. Moreover, in reality near Quito, the roof-top model is far from being perfect. Instead the true model top should have a width of about 10–12 miles. Bouguer then concluded the following: "Therefore we can presume, without any risk of being mistaken, that the effect of the mountain belt reaches its possible maximum, i.e., the half of the effect which would be produced by the complete spherical layer, which we can express as

$$\frac{3}{2}h\delta \tag{3.3}$$

If we add the expression (3.3) to the value of $r\Delta - 2h\Delta$, which is supposed to be caused by the spherical Earth without topography, the gravity at Quito can be expressed as (3.1), while $r\Delta$ expresses the gravity encountered at sea level." Let us now sum up the Bouguer's findings.

We already have mentioned that Bouguer measured the one-second pendulum lengths in Quito (he mentioned Plaza Grande) and on a small island in the stream Inca, a tributary of the present-day Rio Blanco (the latter he then called "rivière des Émeraudes" although today that name, namely Rio Esmeraldas in Spanish, is used for the river originating from the mentioned Rio Blanco and another river, Rio Guayllabamba, some 20 km downstream). In addition, there was the third place, the summit of the Pichincha volcano on the southeastern slopes of which the City of Quito had been established. Based on his geodetic and astronomical observations Bouguer estimated the elevations of those three places. For the river island within the Inca stream it was 40–42 toises (Bouguer, 1749, p. 166) although he originally believed that it had to be about 30 toises (Bouguer, 1749, p. 161); for Quito and Plaza Grande, he found the elevation of 1466 toises; and finally for the Pichincha summit he gave 2434 toises. With 1 toise being equal to 1.949 m, these elevations then were approximately 80, 2857, and 4744 m, respectively. It should be mentioned here that today the summit of the Pichincha volcano has a different shape resulting from eruptions, which have taken place in the meantime so that Bouguer's value of the elevation cannot be checked, while the other two above-the-sea-level elevations seem to be more or less acceptable as approximations, according to the data available today.

In fact Bouguer found that his pendulum was shorter in Quito and shortest at the summit of the Pichincha volcano when compared to its length measured at an elevation close to sea level (i.e. his Inca island station). He had immediately concluded that the force of gravity changed depending on the distance from the Earth center. "That force decreases with increasing elevation" (Bouguer, 1749, p. 357). In Quito he observed that his pendulum was shorter than it had been at the lowest station by 33/100 of the French line (1 line = 2.2558 mm) or by 1/1331. At the summit of Pichincha it shortened by an additional 19/100 of the French line so that it was shorter there by 1/845 compared to its length at the lowest station. We should only add here that his fractions 1/1331 and 1/845 were of course approximate. Bouguer readily recognized that those differences in the pendulum length (i.e., quantities proportional to gravity) cannot be ascribed to the centrifugal force, in spite of the fact that it should act in the required sense. Bouguer estimated the amplitude of its centrifugal component to be much less than the observed difference. He offered expression (3.1) as a way how to estimate the gravity decrease with increasing height of the measuring place. In our opinion, expression (3.1) represented a genuine generalization of those few measured values. Today we can interpret Eq. (3.1) also in terms of the expected or theoretical gravity estimation. Therefore we understand that with his expression (3.1) Bouguer laid the foundation of what has become known today as the Bouguer anomaly as well as the foundation of the gravity method of (applied) geophysics.

It is important to note that the second and the third terms of the expressions (3.1) or (3.1a), namely Eq. (3.2) and expression (3.3), were based on spherical approximation, as well as the first term, as shown earlier. This remains valid even if we acknowledge Bouguer's auxiliary estimations using his roof model as shown in Fig. 3.2. The terms like "the Bouguer infinite slab" or "plate" which are now associated with the third term were in fact developed later after Young (1819) and Poisson (1833) have done their work regarding the well-known expression for the gravitational effect in question, see later. Here we should add that even in the Poisson's approach we cannot see any straightforward replacement of some part of an idealized topography with a spherical base by a (horizontal and infinite) plate or slab. In fact, sometimes the things in the past happened a bit differently than they are seen today.

Immediately after having arrived at the expression (3.1), Bouguer applied it to derive an estimate of the topographic density. Taking the pendulum shortening between the Inca island and Quito 1/1331 and $h/r \approx 1/2237$ he got (Bouguer, 1749, pp. 362)

$$\frac{1}{1331} = \frac{2h\Delta - \frac{3}{2}h\delta}{r\Delta} \Rightarrow \frac{1}{1331} = \frac{1}{2237} \times \frac{2\Delta - \frac{3}{2}\delta}{\Delta} \Rightarrow \delta = \frac{850}{3993}\Delta \quad (3.4)$$

Today we know that $\Delta \approx 5515\,\mathrm{kg/m^3}$ (Cox, 2002) and thus Eq. (3.4) estimates the topographic density as $\delta \approx 1174\,\mathrm{kg/m^3}$, which is not in a good agreement with the possible densities of the topographic masses. Thus from the aspect of our present-day knowledge, Bouguer's estimation of the density of the topographic masses indicated that there should be some other reason for the gravity change between those two places in addition to the ones described by the second and third terms of Eq. (3.1). To sum it up, we can consider the density estimation in Eq. (3.4) as rather unsuccessful. We will discuss this in a greater detail later.

After having discussed the very foundations of our subject we will now proceed a couple of decades onward and discuss some of the most significant issues preceding the establishment of the gravity method as a part of geophysics or applied geophysics.

3.3 REDUCTIONS OF GRAVITY OR THE PENDULUM LENGTH TO THE SEA LEVEL

3.3.1 The So-Called Bouguer Reduction

It is not easy to retrace the beginnings of the historically important concept of reductions of gravity to the sea level which has had such a controversial impact on the development of the Bouguer anomaly concept. Let us begin with the discussion of allowing for the ground masses between the station and the sea level, though historically, it seems to have been preceded by the allowance for the rest of the Earth below the sea level.

For instance Lambert (1930, pp. 137, 138) wrote: "It does not appear, however, that Bouguer recommended for general use what is now known as Bouguer's method. The first use of it as a general method for reducing to sea level appears to be due to Thomas Young who said, in effect, that the free-air method had hitherto been used but that he recommended an allowance for the matter between the station and sea level which matter might to a first approximation be treated as an infinite slab. Poisson also recommended the same method. The method is sometimes and more appropriately named after Young or Poisson, but more often and less appropriately after Bouguer."

We assumed that some of the Lambert's statements quoted earlier might have been at least partly based on Helmert, although Helmert's name was not mentioned here, but, in his "Bibliographical Notes," Lambert (1930, p. 176) refers to some others of the Helmert's works. In fact, in the fundamental textbook of Helmert (1884, p. 166), we can read more or less the same as in the above citation of Lambert (1930). Regarding the terminology associated to the allowance made for the rock-mass between the station and sea level, Helmert (1884, p. 166) wrote the following: "This relation is called the Young rule as well as the expression of Poisson for flat terrain. Anyway we will call it after Bouguer who was the first to study such relations." So, we can ask—was the technical term "Bouguer reduction" first introduced by Helmert? Likely it was. And, moreover, it is interesting to note that this happened probably merely thanks to the geodetic concept of reducing gravity from the Earth surface to the sea level.

According to Helmert (1884) and Lambert (1930), Young (1819) seems to be the first one who considered the attraction of rock masses between the gravity station and the sea level. But this has not necessarily to be the case and there are at least two reasons for some doubt. First, Young's own words (Young, 1819, p. 93) "… for example, in the allowance made for the reduction of different heights to the level of the sea, which has usually been done without any consideration of the attraction of the elevated parts, interposed between the general surface and the place of observation … ." He actually wrote "has usually been done" instead of, say, "has always been done." Second, Todhunter (1873b, p. 490) wrote that "Dr. Young's rule … coincides with the formula originally given by Bouguer and reproduced by D'Alembert … Dr. Young does not refer to any preceding writer." It is then obvious that Young had not a good reputation regarding quoting his predecessors. As a consequence, knowing that Bouguer (1749) did not reduce his gravity measurements to the sea level, to decide whether Young (1819) was really the first to use this kind of reduction would require further investigation.

Still Young's estimations were very interesting. Young (1819, p. 93) continues: "It is however obvious, that if we raised on a sphere of earth

$$k' = 2\pi f\rho'(c + h - \sqrt{c^2 + h^2}),$$

en désignant cette force par k'. Mais, en général, l'épaisseur verticale de la couche attirante est petite, eu égard à son rayon horizontal ; si donc on néglige h^2 par rapport à c^2, on aura simplement

$$k' = 2\pi f\rho' h.$$

Figure 3.3 Poisson (1833) estimating formula. From his original sketch, however, we can see that he had actually in mind rather a model bounded by two spherical surfaces than a slab. The infinite horizontal slab and its calculated attraction, were in fact Poisson's approximations to the volume depicted in his Figure 59 and to its corresponding gravitational attraction.

a mile in diameter, its attraction would be about 1/8000 of that of the whole globe, and instead of a reduction of 1/2000 in the force of gravity, we should obtain only 3/8000, or three fourths as much ...." And finally: "Supposing the mean density of the Earth 5.5, and that of the surface 2.5 only, the correction, for a tract of table land, will be reduced to $1 - \frac{3}{4} \times \frac{2.5}{5.5} = \frac{29}{44}$, or 66/100 of the whole." After recalculation based on the present-day data his fractions become: $\frac{1}{8000} \to \frac{1}{7931}$, $\frac{1}{2000} \to \frac{1}{1975}$, but $\frac{66}{100} \to 0.66056$! The last figure deserves special attention: $0.3086 - 0.04193 * 2.5 = 0.20385$ and $0.20385/0.3086 = 0.66056$. It is evident that in estimating the impact of the attraction of the rocks between the observation point and the sea level Young achieved a considerably high accuracy.

Poisson (1833), on the other hand, is known to have derived the formula which has since been in use and which obviously supported the "introduction" of the concept of an infinite horizontal slab, or simply Bouguer slab, into geodetic and gravimetric practice (Fig. 3.3).

We conclude that, as a matter of paradox, neither Bouguer nor Young and even Poisson primarily considered the model of a horizontal infinite plate. The impression remains that as if they all were standing on "terra firma sphaerica." Therefore it is not simple to ask who introduced the "Bouguer plate" model and when it happened—it is rather difficult to find.

3.3.2 The So-Called Free-Air or Faye Reduction

When the free-air reduction was introduced into geodetic practice? Heiskanen and Vening Meinesz (1958, p. 150) tried to explain why this reduction often points to the name of Faye and then gave some time frame. They wrote: "The free-air reduction is often called Faye's reduction after the man who called attention to it. ... This kind of reduction was frequently used in the eighteenth century and at the beginning of the nineteenth; important work of Stokes and Faye was based on it." In fact Stokes (1849, p. 673) refers to it: "... and observed gravity be reduced to the level of the sea by taking account only of the change of distance from the earth's centre." But should Faye's work be considered important from the aspect of the free-air reduction? We are not sure. From the Faye's contributions one can deduce that this kind of reduction had been in use a long time before he wrote his memoirs, and that Faye himself apparently did nothing in order to either strengthen or weaken it. On the other hand, Faye did not want the attraction of the intermediate rock being subtracted from the measured gravity. He wrote: "The continent has been compensated, almost entirely, by disturbances in thickness of the rigid crust under the continents and therefore it is not necessary to account for it ..." (Faye, 1880b, pp. 1443, 1444). Here he also referred to his earlier paper (Faye, 1880a) where he had pointed out this concept for the first time. It is really interesting to read what he wrote later (Faye, 1880b, p. 1445): "It is however necessary to realize that, if the thickness of the continents above the sea level will not be taken into consideration, it would not be the same as, for instance, the mass of the Great Pyramid in Egypt, provided that the pendulum measurement were performed at its summit. So, after the reduction of the pendulum length at the sea level according to formula (3.5)

$$l_0 = l + \frac{2hl}{R} \tag{3.5}$$

where l_0 is the pendulum length reduced at the sea level, l is the length measured at the pendulum station at the elevation h above the sea level, and R is the Earth radius; it will be necessary to subtract the attraction of the pyramid above the Earth surface. Likewise, when Bouguer brought his pendulum at the summit of the Pichincha volcano about 1500 m above the level of the terrain in Quito, he should have accounted for the attraction of this mountain on his pendulum." In his subsequent papers Faye (1883, 1895) did not seem to introduce anything different with regard to what we have discussed here. There were some other concepts of Faye, however, which we will briefly mention and comment later.

To sum up, we have not been successful in finding any clue regarding the earlier development of the free-air reduction within either the papers of Young (1819), whom we already quoted earlier and Stokes (1849) or within the memoirs of Faye (1880a,b, 1883, 1895), or elsewhere.

3.3.3 When and by Whom the Reductions to the Sea Level Were Introduced Remains at Least Partly Unknown

Was Thomas Young really the first using what is now known in literature as Bouguer reduction? And who was the first using the so-called free-air or Faye's reduction? Those questions remain unanswered at present. We know of course that the background of both reductions come conceptually from Bouguer (1749) with only one important issue to be repeated here—Bouguer did not intend using those procedures in order to reduce his Quito or Pichincha pendulum lengths to the sea level. This should be kept in mind.

3.3.4 The First Initiative Against Reductions to the Sea Level

Hayford and Bowie (1912) seem to be the first specialists who looked at the problem of reductions differently. After they calculated the theoretical value of gravity at the sea level they computed "the correction for elevation … of the station" according to simple expression (3.6), values in dynes, elevation H in meters,

$$-0.0003086H \tag{3.6}$$

which has negative sign, likewise the Bouguer's second term in expression (3.1) (Hayford and Bowie, 1912, p. 13), but in contrast to the positive sign used, e.g., in formula (3.5) of Faye. The correction (3.6) is applied to the value calculated according to Eq. (3.7), see later. It is obvious that "… (3.6) is the reduction from sea level, to the station, a correction to the theoretical value not the observed value. It takes account of the increased distance of the station from the attracting mass, the earth, as if the station were in the air at the stated elevation and there were no topography on the earth" (Hayford and Bowie, 1912, p. 72).

Before we continue with the discussion of their next related step, i.e., their allowing for topography, we should note that Hayford and Bowie (1912) had decided to (1) correct for the topography and its isostatic compensation simultaneously, and to (2) take into consideration the whole globe up to the antipodes of the pendulum station. Having this in mind we can continue the previous quotation: "The correction for topography and compensation was computed with the new reduction tables. This is also a correction to be applied to the theoretical value at sea level." The latter statement is in fact not easy to understand since the authors write elsewhere (Hayford and Bowie, 1912, p. 28): "All tabular values are the vertical components of the attraction upon a unit mass at the station ...." The only plausible understanding seems to be that their phrase "theoretical gravity at sea level" represents the term they use for the output of the Helmert's well-known formula for γ_0 derived in 1901, namely

$$\gamma_0 = 978.046(1 + 0.005302 \sin^2 \phi - 0.000007 \sin^2 2\phi) \qquad (3.7)$$

(Hayford and Bowie, 1912, p. 12; values of γ_0 are in dynes and ϕ is termed as "latitude") and that this does not mean that the correction for topography and its compensation should be applied at the sea level. Hayford and Bowie (1912, pp. 72, 73) continue: "Usually corrections are applied to the observed values of the intensity of gravity to reduce them to sea level and to correct for the supposed influence of topography. In this publication the corrections are applied to the theoretical value of the intensity of gravity at sea level to obtain the theoretical value at the station, a value which is directly comparable with the observed value. This seems to the authors to be a more logical method and more conductive to clear thinking than the usual method." In the times to come, many specialists have agreed the above-quoted approach is the only one compatible with geological interpretation of gravity data. On the other hand, there have been also many who either ignored it or even openly continued in supporting the original reductions to the sea level. We will give some brief examples later.

Bullard (1936) uses the term "reduction" or "to reduce" at least in three different meanings of which we will quote corresponding examples within Section 3.4.8. On the other hand, although he mentions the work of Hayford and Bowie (1912) quite frequently, he does not comment their refusing to reduce the measured gravity to the sea level. In fact he has not touched the problem of such reductions in the Bullard (1936) paper at all. Notwithstanding the assumption that he calculated and interpreted his anomalies as station quantities can be based on other kind of evidence. For instance, Bullard (1936, p. 501) writes: "The observed and calculated values of g at all the stations ...."

In addition, he evidently considers the uneven Earth surface in his interpretation (Bullard, 1936, p. 507 and following pages). Assuming this, however, we must consider his quantity called "the difference between gravity at sea level and at the height, h, of the station, neglecting the attraction of the topography" equal to "$+0.3086 \times 10^{-3}h$," h being in meters (Bullard, 1936, p. 501), as inconsistent since it should have been negative had he understood the problem as Hayford and Bowie (1912) did.

3.4 ADDITIONAL DISCUSSION

3.4.1 Bouguer (1749)

3.4.1.1 Some Interesting Quotations

Let's go back to Bouguer's own text. He among others wrote (Bouguer, 1749, p. 357): "The experiments with the pendulum which we performed in Quito, as well as at the summit of the Pichincha mountain, tell us that gravity is changing depending on the distance from the Earth centre."... "However, the measured differences cannot be ascribed to the centrifugal force." Perhaps this was the first experimental confirmation of decreasing gravity with elevation, which had been predicted earlier by Newton. Later Bouguer (1749, p. 358) continued: "But why our experiments all the time yield a relation which does not completely satisfy the quadratic condition?" And subsequently on the same page we can read: "We will possibly find the solution to this difficulty if we notice that the Cordillera is forming something like an 'other Earth' and, from some aspects, this must be the same as if the Earth surface was moved to the higher elevation or to the greater distance from the Earth centre."

3.4.1.2 Prediction or Reduction?

We understand that Pierre Bouguer was capable of predicting gravity at elevated stations. On the other hand, he evidently had no ambition of reducing the values measured at the tops of the topographic forms to any "datum plane" or to the sea level. Actually, by his expression (3.1) or (3.1b) he in fact offered a way how to estimate what we would now call the theoretical gravity, namely by calculating the gravitational effect of a "normal Earth" though today we would probably not accept the effect of the homogenous sphere as the first or the principal component of it (see Section 3.5.1.3). To sum it up: although we have found mentions about reducing the distances measured for determining the length of a meridian degree at sea level (Bouguer, 1749, p. 167 and following) we did not come across any mention of reducing gravity (or pendulum length) to sea level within Bouguer's book. On the other hand, Heiskanen and Vening Meinesz (1958, p. 153) saw things a bit differently: "The effect of the Bouguer's plate diminishes the effect of the free-air reduction by about one-third. This reduction was originated by Bouguer, who derived the formula in his work "La figure de la terre" in 1749. He used this reduction to compare the gravity values observed on the plateau of Quito and the neighboring seacoast of Peru." We interpret this final statement as being just the authors' assumption and actually this was not the case. We can only repeat that in fact we have found no indication, direct or indirect, that Bouguer (1749) wanted to compare his pendulum lengths reduced to the sea level.

3.4.1.3 Bouguer's First Term

Bouguer's prediction was based on estimating the gravitational effect of his "normal Earth" consisting of a sphere and the topography. His sphere had the radius of about 6391632 m, was characterized by constant density Δ and the gravitational effect at its surface was given by the first term of expression (3.1b). In contrast with the ellipsoid of revolution which we use today and which represents a surface with constant gravity potential but with different values of gravity at different latitudes (i.e., equipotential surface), the surface of the Bouguer's sphere not only had the same gravitational potential (as we would say today) but also the same gravitation at its surface. Thus Bouguer's "normal Earth" was equigravitational (neglecting the centrifugal component). We realize that the mentioned qualities of the ellipsoid and especially of the approximation sphere may seem unimportant yet we are convinced that their impact was in reality quite serious as we will see later in our discussion of Bouguer's density estimate.

3.4.1.4 Bouguer's Second Term: The Free-Air, Faye, or Height Correction/Reduction Is Actually Due to Bouguer

The second term of Eq. (3.1) or (3.1b) represents what is now known as the free-air, Faye or height correction/reduction. Therefore it should be credited to Bouguer as identified by Bullen (1975). If we use the present-day values of γ and Δ and if we add the centrifugal component into the second term of Eq. (3.1b), we even obtain remarkably similar values compared to GRS80 calculations (Moritz, 1988; see also Chapter 4, Normal Earth Gravity Field Versus Gravity Effect of

Layered Ellipsoidal Model, Table 3.). Surprisingly, one can find a reference to the fact that Bouguer's second term represents the free-air correction in the literature only scarcely (e.g., Putnam, 1895; Bullen, 1975, as quoted earlier), while the vast majority of recent textbooks do not mention it.

3.4.1.5 Bouguer's Third Term

Retrospectively, we should consider Bouguer's estimation of the gravitational effect of the topographic masses by the third term of Eq. (3.1) or (3.1b) as excellent. The quantity we now call the "Near Topographic Effect" (*NTE*, as a counterpart to the Distant Topographic Effect or *DTE*, Mikuška et al., 2006; see also Chapter 5, Numerical Calculation of Terrain Correction within the Bouguer Anomaly Evaluation (Program Toposk) can be well approximated by this term as illustrated in Fig. 3.4. In reality the real *NTE* is

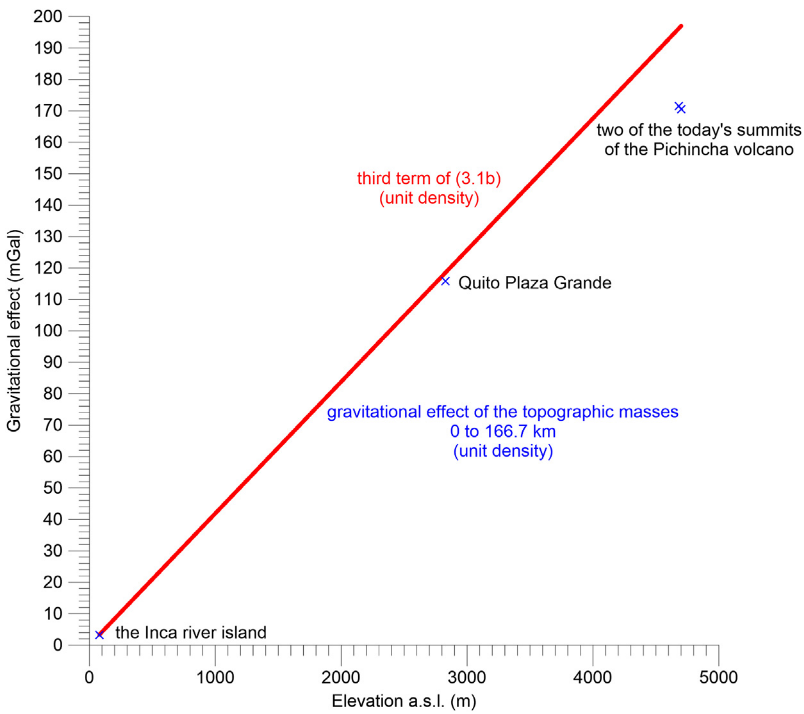

Figure 3.4 The estimation of the gravitational effect of the topographic masses by the third term of the expression (3.1b) (red line(dark gray in print versions)) and its values calculated from the available DEM models (blue crosses(light gray in print versions)) using the program Toposk (Marušiak et al., 2013; see also Chapter 5, Numerical Calculation of Terrain Correction within the Bouguer Anomaly Evaluation (Program Toposk).

proportional to h in the sense that Bouguer's third term represents approximately its upper limit. The third term of Eq. (3.1b) coincides well with the actual *NTE* calculated for Quito, i.e., the place where this approximation had been introduced.

3.4.1.6 Some of the Reasons Why Bouguer's Density Estimation of the Topographic Masses Was Unrealistic

Bouguer's density estimation was in fact unconvincing. We regard three reasons as having special importance: (1) There was poor general knowledge about the real rock densities or the density structure of the Earth in the half of the 18th century, and, instead, there were lots of "density speculations." (2) The gravitational constant had not yet been recognized or measured and, as a result, Pierre Bouguer could not confront the real full-valued differences between the measured and expected pendulum lengths with his prediction to which he had arrived at by combining the three terms of expression (3.1); obviously he only could calculate fractions. (3) Possibly even more fundamental, he was not aware that subtopographical lateral density changes can quite dramatically change the gravitational effect which he calculated on his approximation sphere surface. He simply supposed (in fact he had to suppose) that the spherical part of the real Earth would always behave like the spherical part of his "normal Earth" model, i.e., as if it were equigravitational. Unfortunately, this was rather far from geological reality. To avoid the effects of those lateral density changes, Airy (1856), von Sterneck (1883), and subsequently many others, tended to move into vertical mining shafts with their measurements aimed at the density estimations.

3.4.2 Faye (1880−95)

While we consider Faye's association with the introduction of the free-air correction to be slightly controversial, Faye deserves full credit for his other contributions to the gravity method.

Faye (1880b, p. 1446) uses the word "anomalies" in a direct relation to gravity values (more accurately to the pendulum lengths). We can then trace back the concept (and the term) of "gravity anomaly" as far as to 1880. In his 1883 memoir (Faye, 1883, p. 1261) he uses the term "Poisson's correction" for what Helmert (1884) recommended using "Bouguer reduction" as we already wrote earlier. Faye (1880b) seems

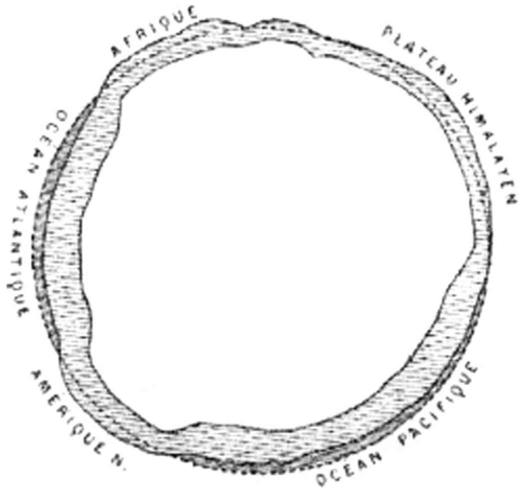

Figure 3.5 Faye (1895) obviously expected thicker crust under oceans and thinner under mountain ranges (his Figure 2 on his page 1085).

to have firstly dealt with modern isostasy concepts, even if he ends up with wrong conclusions later on (Faye, 1895; Fig. 3.5).

3.4.3 Helmert (1884)

Without any doubt Friedrich Rudolf Helmert was an outstanding scientist with enormous influence on the contemporary as well as the next generations of geodesists. This is well demonstrated by Putnam (1895, pp. 43, 44, 46, 56) or Hayford and Bowie (1912, p. 12), for example. However, if we are to consider Helmert's contribution especially from the perspective of the history of the Bouguer anomaly, we find that it should be viewed rather questionable.

There was, for instance, the way how he described condensations and reductions. Regarding the latter, Helmert (1884, p. 162) wrote: "The measurements of the acceleration of gravity actually apply for points at the physical Earth surface and thence they have to be reduced to the sea level in order they could be considered as belonging to one level plane." Such description then would logically have its impact not only upon geodesists but later on also upon geophysicists, for decades to come after publication of the Helmert's textbook. Although Helmert probably meant the quoted phrase as related to the "geoid computations," likewise it could easily be (mis)understood as "valid generally." However the same time it should be stressed here that not

Helmert (1884), but geophysicists themselves have in fact been responsible for the negative interpretational impact of the idea of "belonging to one level plane."

We perceive the notion of reductions very important from the aspect of our topic and we believe the following comment will not be redundant. Helmert (1884, p. 166) provides the expression

$$\left(1+\frac{2h}{r}\left[1-\frac{3}{4}\frac{\delta}{\Delta}\right]\right)g. \tag{3.8}$$

In Eq. (3.8) we exchanged his original symbols by those used by Bouguer (1749) (see expression (3.1)), except for the symbol g which is new in Eq. (3.8) and represents, according to Helmert, the measured gravity. It is clear that Eq. (3.8) was intended to describe "the traditional reduction of the gravity measurements to the sea level" (Helmert, 1884, p. 163). Here Helmert proposes to call the reduction after Bouguer as we mention before. It is, however, trivial to realize that if we multiply Eq. (3.8) by the factor $r\Delta/g$ we get Bouguer's expression (3.1) with changed signs of both the second and third terms. But then there could be a logical question: Provided that Eq. (3.1) is considered (formally) correct, can Eq. (3.8) be considered (formally) correct as well? We think it cannot be. The reason is that multiplication by variable g in Eq. (3.8) would mean that the change of gravity should depend on (variable) gravity itself, which would hardly have a physical reason especially regarding the third term. At least Helmert (1884) did not mention anything that could be considered a physical explanation.

Helmert (1884, p. 179) discusses a topic which has something in common with what we would call today the bathymetric correction. Helmert's reasoning is interesting but it is rather difficult to follow since it is amalgamated with his condensations. Further, on his pages 227–229 he criticizes the approach of Faye (1880b, p. 1444, see our discussion regarding Eq. 3.5) to the problem of compensation of local topographical features. This criticism of Helmert we consider substantiated since Faye (1880b, p. 1444) did not very much care about detailed specifications.

3.4.4 Putnam (1895)

Putnam (1895, p. 43) reproduces the formula, which was given by Helmert (1884) (our expression 3.8) when describing the process of "reducing pendulum observations to the sea level," and calls it Bouguer reduction according to Helmert (1884). On the other hand, Putnam considers *g* as gravity at sea level, contrary to Helmert. This is interesting. Here Putnam is closer to Bouguer (1749) than to Helmert (1884).

Further Putnam (1895, pp. 43, 44) applies the corrective term for the situation "whenever the topography about a station departs materially from this condition" and calls it "topographical correction" which should be always positive as he writes. Here Putnam quotes Helmert (1884, p. 169). We can only add that the concept of terrain correction (today the term "terrain" is preferred rather than "topographic") was probably introduced 40 years earlier by Peters (1855, p. 46).

Putnam (1895, pp. 53, 54) uses, discusses, and criticizes the Faye's method or Faye's reduction (1880b) while later Faye (1895) calls it correction. Please note that this method (reduction) of Faye is different from the free-air or Faye's reduction (correction) as we refer to elsewhere.

3.4.5 Hayford and Bowie (1912)

It is obvious that both the Earth model below sea level (including sea water and compensation of topographic masses) and the topographic masses themselves contribute to the theoretical gravity of Hayford and Bowie (1912).

In addition, it should be stressed that their approach was revolutionary also from other aspects as we briefly mentioned earlier. Hayford and Bowie (1912, p. 72) wrote: "It would be difficult to show satisfactorily by pure theory without numerical values why and to what extent the *curvature* and *distant topography* and compensation must be considered. … Instead the computations have been made to cover the whole earth by formulae which are practically exact, curvature being adequately taken into account. This having been done the numerical results … demonstrate conclusively and clearly that both distant topography and curvature must be considered if one is to secure even a fair approximation to the truth."

In the last quotation we did not highlight compensation since it should be considered an assumptive issue while the other two, namely the earth curvature and distant topography, were undoubtedly pure reality. In fact they combined together compensation and topographic effects, i.e., the assumptive and nonassumptive components, and that was unnecessary. We think it would have been much more logical if they had accounted for the topography and its compensation separately.

Among the constants introduced by Hayford and Bowie (1912) we can recognize three with the greatest importance, namely (1) the density of the near-surface crust: 2670 kg/m^3, (2) the outer limit of the near topographical masses with regard to a gravity station: 166.7 km, and (3) the depth of compensation: 113.7 km.

1. "... the mean density of the solid portion of the earth for the first few miles below the surface is assumed in this investigation to be 2.67" (i.e., 2670 kg/m^3; Hayford and Bowie, 1912, p. 10). More details about the background of this value, its adoption in geophysics as well as how Hayford (1909) and Hayford and Bowie (1912) worked with it is given in Hinze (2003). As a matter of fact, the assumption that the topographic masses have the density of 2.67 (something like a global average) has outlasted until today, although some different values have been used regionally, and although the concept of variable densities has been introduced in the meantime (Vajk, 1956, and many others).
2. The distance of 166.7 km from the gravity station (the outer limit of their zone O) was introduced by Hayford and Bowie (1912) as the boundary between their near and distant zones but they did not explain their choice. This figure (166.7 km) has also been in use until today. Bullard (1936) in fact introduced its use as an outer limit for allowing for topography, although Hayford and Bowie (1912) recommended to calculate the topographical effect around the Earth.
3. Regarding their compensation depth we can quote: "In the computations of the investigation here published the depth of compensation is assumed to be 113.7 kilometers under every separate portion of the earth's surface" (Hayford and Bowie, 1912, p. 10). Unlike the other two of their figures mentioned earlier, this compensation depth already does not seem to be "on duty." It is likely that this has happened at least partly because the Pratt–Hayford isostatic system as such has been generally less and less in use since there have been strong arguments against it (e.g., Glennie, 1932, p. 26 and elsewhere; Evans and Crompton, 1946, p. 215 and elsewhere).

Within the editorial discussion devoted to the last quoted paper one can find even the following statements. "… the speaker hoped that it would soon be possible to say without shock to the followers of Hayford and his apostle, Bowie, that large areas were not in isostatic equilibrium …" (Holland in Evans and Crompton, 1946, p. 246). Or … "the attention given to Pratt's hypothesis (perhaps because it lent itself more readily to mathematical treatment) was unfortunate, and that it would have been better if it had never been put forward" (Evans in Evans and Crompton, 1946, p. 249). And, in addition, the recent independent studies like CRUST1.0 (http://igppweb.ucsd.edu/~gabi/crust1.html) do not generally support the density distribution which would be required if such an isostatic system would have worked. In this light the statement of Hinze et al. (2013, p. 139), namely that "… in some cases the forces derived from these mass variations exceed the elastic limit of the lithosphere, leading to localized isostatic compensation as suggested by the Pratt-Hayford hypothesis" would require further specification.

3.4.6 Bullard (1936)

From the aspect of methodology (and history of our subject of course) the paper of Bullard (1936) represents something between what "had been" and what "has been." It is generally regarded as a milestone although in fact it brought little of what could be considered new from the aspect of methodology except for a few small changes or improvements.

Among others, Bullard criticized the way how Hayford and Bowie (1912) calculated the attraction of the topography between the station and their zone O. On his page 487 he wrote: "The work may be very much reduced if the attraction of a plateau on whose surface the station lies and which stretches to zone O is first calculated, and the difference between this and the attraction of the actual topography calculated by means of tables." Bullard did not mention, however, that a similar two-step method had already been proposed by Helmert (1884, pp. 169–172) and Putnam (1895, pp. 43, 44). The mentioned tables were those published by Cassinis and Doré (1933). Bullard calculated the attraction of the compensating masses within the inner zone according to the tables published by Heiskanen in 1931 and 1932. For saving work time Bullard sometimes combined the approaches of Heiskanen and Hayford. He considered the attraction of topography and compensation beyond zone O similarly as Hayford and Bowie (1912) by calculating the combined effect for all but one of his 87 pendulum stations.

To eliminate the evident discrepancy between the then used horizontal infinite slab and the real (nearly spherical) Earth he introduced his "slab extending to the outside of zone O and curved to the radius of the earth" (Bullard, 1936, p. 487) which we would now call truncated spherical layer or shell, with thickness equal to the station height. Bullard introduced an auxiliary term which, if added to the gravitational effect of a horizontal infinite slab, changes it into the effect of a "curved slab." This term now bears Bullard's name, being called either "Bullard term" or "Bullard B" (LaFehr, 1991). However, all this can be viewed as a rather complicated issue. Bullard (1936), along with many others later on, does not seem to have acknowledged that Bouguer (1749), when allowing for the (near) topographic masses, in fact did account for the Earth curvature, considering his "half of the effect which would be produced by the complete spherical layer" as we quote him earlier when discussing expression (3.3). So there is an obvious difference between the approaches of Bullard (1936) and Bouguer (1749), respectively, which we would like to point out here. We mean that while Bullard's maximum angular distance or outer limit (the one adopted from Hayford and Bowie, 1912, namely $1^\circ 29' 58''$) remains always constant and is independent on the height of the actual measuring point h, Bouguer's "maximum angular distance" had to be dependent on h, and thus varying from point to point. Let us recall that Bouguer, for his approximation of the (near) topographic masses, did use neither infinite plate nor (truncated) spherical layer but spherical cap. However, it should be noted here that Bouguer (1749) had not discussed the question of the maximum angular distance or outer limit explicitly. Nevertheless, until today, the procedure has remained the same as it was introduced by Bullard (1936).

Interestingly from the historical aspect, Bullard (1936) quotes Helmert only seldom. This happens on his page 487 and here it is associated to the earlier gravity work in East Africa prior to Bullard's measurements, and then on his page 501 and following in connection with Helmert's formula for γ_0 which is identical with the one which we reproduce as our expression (3.7) except for the used equatorial value. On the contrary, Hayford and Bowie (1912) quote Helmert more frequently.

And Putnam (1895) did so yet more often. On his page 56 he even quotes a passage from Helmert (1890) in German which we consider very illustrative. With some caution we can interpret this matter as a slight shift from more or less geodesy to more or less geophysics, within approximately one or two human generations of the elapsed time.

3.4.7 Abandoning Reductions to the Sea Level

After the works of Hayford and Bowie (1912) and also Bullard (1936) were published, one would expect that those questionable reductions would either disappear from the geophysical literature or, at least, their occurrence would be less and less frequent. Unfortunately, this has not been the case.

For instance Heiskanen and Vening Meinesz (1958, p. 147) wrote: "Before we use the observed gravity values for practical purposes or for theoretical studies, we must reduce them in a proper way to the same level, usually to sea level or the geoid." We interpret this statement as a misunderstanding of the authors who had been, no doubt, two of the topmost specialists.

On the other hand the approach that gravity anomaly should be understood as a station anomaly was outlined and stressed for instance by Grant and Elsaharty (1962, p. 616), Tsuboi (1965, pp. 386, 387), and Naudy and Neumann (1965, pp. 2, 3).

LaFehr (1991) drew attention to this problem again and did what was possible to explain and to demonstrate the fatal error we can commit when not avoiding this kind of reductions. He wrote (LaFehr, 1991, p. 1177): "How widespread this notion is can be seen by reviewing the major textbooks on the subject: of 15 English-language books which carry descriptions, no fewer than nine state or imply" ... "that our intent is datum reduction." He then quotes three of the textbooks giving correct explanations of what he called "the data reduction process."

Later on Li and Götze (2001, p. 1660) wrote, feeling that there was still some confusion among geophysicists regarding the sense of the free-air reduction: "However, the 'free-air' reduction was thought historically to relocate gravity from its observation position to the geoid (mean sea level). Such an understanding is a geodetic fiction, invalid and unacceptable in geophysics."

More recently the fact that gravity anomaly should be related to the gravity station was stressed among others by Hinze et al. (2005, p. J28), to quote the publication, which has been coauthored by as many as 21 prominent, mostly North-American, geophysicists.

However, even today the confusion is still present. For example, although referring to Hinze et al. (2005), Mallick et al. (2012, p. 5) wrote: "When the gravity observations are made at two stations, each located at different elevations, there would be a difference in the two gravity readings at these stations, which if not corrected for, might indicate a spurious sub-surface structure. This variation in the gravity measurements can be removed by introducing a datum plane with a certain elevation above sea level. *All the material above the datum plane is mathematically removed so that the instrument can be effectively imagined to be placed on top of the datum surface.*"

Unfortunately Mallick et al. (2012) are not the only ones who recently published an incorrect approach to gravity reductions in applied (exploration) geophysics. Long and Kaufmann (2013, pp. 25, 29) represent another example, although they also refer to Hinze et al. (2005). On the other hand, for instance, the approach of Dentith and Mudge (2014, pp. 103, 104) is correct.

In the light of the above mentioned the only possible outlook from our historical hindsight is that the struggle against reductions to the sea level should continue.

3.4.8 Just Three Terminological Comments

Above all we would like to discuss the term "reduction" or "reduce" which has been quite frequently used in a variety of meanings. For instance in Bullard (1936, p. 450) that term was used for lessening the number of some specific measured quantities. Further Bullard (1936, p. 487) writes about the tables of Hayford and Bowie (1912): "… first the attraction of the topography in the compartment on a station at the same height as the mean level of the compartment is taken from the main table, then the correction necessary to reduce this to the attraction at the actual height of the station is found from a subsidiary double entry table" and immediately after he uses that term for description of saving work. A different meaning can be found in Putnam (1895, p. 52): "… Apply further correction to the observed force of gravity … to reduce to the normal condition." Bouguer (1749) uses this term in connection with projection of the measured distances on the sea level as we quote earlier ... Young, Stokes, Faye, Helmert, and many others used this term when describing the relocation of gravity values.

From our brief (and thus necessarily incomplete) review it follows that the term "reduction" has been in use not only in the sense of (from the geophysical point of view invalid) moving gravity values along the local vertical but also in a number of other senses. We thus consider it a most problematic and troublesome terms in gravimetry.

Then there is "Bouguer anomaly." In Bullard (1936) that term has been used more or less in the sense as we use it today. In Hayford and Bowie (1912), however, Bouguer anomaly is understood as a quantity defined on a flat Earth. Putnam (1895) calls the same quantity "residuals with Bouguer reduction" (together with "residuals with reduction for elevation" in the case of free-air anomaly).

And finally there is the term "free-air." For instance either Stokes (1849) or Faye (1880a,b, 1883) had not used this term for the reduction in question while Helmert (1884, p. 166) had (his term "in freier Luft"). It can be of some interest that the term "free-water anomaly" was introduced one century later (Luyendyk, 1984), as an apparent analogy to the "free-air anomaly." It may be important to note, however, that the term "free-air" should be understood in the sense "as if there were air while in fact there was rock," not "there was in fact air and no rock." In this light Luyendyk's "analogy" does not seem very fortunate. Or, is not the term "free-air" also trouble-causing?

3.5 CONCLUSIONS

From the present-day aspect, the development of the Bouguer anomaly concept seems to be a never-ending story. As if it was an object of a complicated evolution rather than a result of some purposeful plan or idea. In addition, it has been closely entwined with geodetic concepts which have proved to be geophysically unintelligible.

As we already indicated earlier, we have decided to terminate our historical excursion approximately at the time of appearance of the paper of Bullard (1936). And we have learned that, until then, some concepts developed differently than we thought we know today.

For instance Bouguer realized already in 1749 that both "mass or rock" and "free-air" components affect the gravity change when the observer moves from lower to higher elevations. Today we usually call those quantities as corrections. The former bears Bouguer's name probably thanks to Helmert who used it for relocating the measured gravity values what Bouguer in fact neither did nor proposed. The latter is either called "free-air" or "Faye" correction. The idea undoubtedly came from Bouguer although Bouguer (1749) never used the term "free-air" which, in turn, has been probably coined by Helmert (1884). And the idea of "free-air" upward diminution of gravity definitely did not originate from Faye.

Faye, on the other hand, promoted the isostatic compensation of the topographic masses in 1880 which was earlier than Fisher (1881) and Dutton (1882, 1889) published their works. For example, Fisher and Dutton are quoted in Watts (2001, pp. 15–17), while Faye is not. Faye, however, did not use the term "isostasy." In this aspect Faye (1880b) seems to have been omitted not only by Watts (2001); in fact we have not found his isostatic thoughts mentioned anywhere.

Bouguer in fact did not introduce the "Bouguer slab" or "Bouguer plate" as we showed earlier.

Although it has been well known that Hayford and Bowie (1912) changed the sense of applying the Bouguer and free-air (sometimes called Faye) corrections on the theoretical instead of the measured gravity, unfortunately this change has not been fully acknowledged. As we have showed, the related misunderstandings have not yet been eliminated.

At the very end of our retrospective we have to highly appreciate once more the contribution of Pierre Bouguer who, as early as it was possible, learned much of what was necessary in order to establish the concept of the gravity anomaly. He introduced a simple normal Earth consisting of an equigravitational sphere plus topography and, on this basis he was capable of predicting gravity at elevated places compared to its value near the sea level. He well realized the role of centrifugal force and distinguished between gravitation and gravity. On the other hand, he seems to have disregarded the possible influence of bathymetry and, what was in our view even more important, he in his time had no means how to interpret the difference between the measured and expected pendulum lengths in terms of lateral density changes within the subtopographical volume of the real Earth. This led him to questionable topographic density estimation. However, we can conclude that Pierre Bouguer had laid the foundations of the present-day Bouguer anomaly house. Yet in his times he could not raise the walls, since not all of the necessary bricks he had in his hands.

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